CHEMICAL HERITAGE FOUNDATION

MORRIS TANENBAUM

Transcript of an Interview
Conducted by

David C. Brock and Christophe Lécuyer

at

Bell Telephone Laboratories, Inc.
Murray Hill, New Jersey

on

3 May and 26 July 2004

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Morris Tanenbaum

(Date) August 5, 2011

MORRIS TANENBAUM

1928	Born in Huntington, West Virginia, on 10 November

Education

1949	A.B., Chemistry, John Hopkins University, Baltimore, Maryland
1952	Ph.D., Physical Chemistry, Princeton University, Princeton, New Jersey

Professional Experience

	Bell Telephone Laboratories
1952-1956	Chemistry and Chemical Physics Division, Technical Staff
1956-1962	Assistant Director of Metallurgical Department
1962-1964	Director, Solid-State Development Laboratory
1975-1976	Executive Vice President, Systems Engineering and Development
	Western Electric Company
1964-1968	Director of Research and Development
1968-1972	Vice President, Engineering Division
1972-1975	Vice President, Manufacturing: Transmission Equipment
	The AT&T Company Laboratory
1976-1978	Vice President, Engineering and Network Services
1980-1984	Executive Vice President
	New Jersey Bell Telephone Company
1978-1980	President
	AT&T Communications
1984-1986	Chief Executive Officer and Chairman of the Board
	AT&T Corporation
1986-1988	Vice Chairman, Finance
1988-1991	Vice Chairman, Finance and Chief Financial Officer

Selected Honors

1970	Institute of Electrical and Electronics Engineers (IEEE) Fellow
1972	Vice President and member of the National Academy of Engineering

1975	ASM Campbell Lecturer
1980	New Jersey Institute of Technology Honorary Doctor of Science
1981	Seton Hall University Honorary Doctor of Science
1982	Stevens Institute of Technology Honorary Doctor of Engineering
1983	Worcester Polytechnic Institute Honorary Doctor of Science
1984	IEEE Centennial Medal
1990	Fellow, American Academy of Arts and Sciences
1992	Lehigh University Honorary Doctor of Science
1996	Elected Life Member of MIT Corporation
1999	John Hopkins University Heritage Award

ABSTRACT

Morris Tanenbaum grew up in Huntington, West Virginia, one of three children. His Jewish parents had come from Russia and Poland by way of Buenos Aires, Argentina; they owned a delicatessen, in which Morris worked after school. He liked and did well in school, always interested in science. A trip to the 1939 World's Fair further focused his interest in science. He graduated from high school and chose Johns Hopkins University because of its reputation for chemistry. He liked physical chemistry and physics and somehow found himself often being the business manager of college organizations. One of his professors, Clark Bricker, who was leaving for Princeton University, convinced Tanenbaum to accept a research assistantship there and to obtain a PhD. Tanenbaum worked on spectroscopy in Bricker's lab. He married Charlotte Silver whom he had met during his sophomore year at Johns Hopkins. For his thesis, he moved to Walter Kauzmann's lab to study the mechanical properties of metal single crystals; he won the DuPont and Proctor Fellowships.

After being awarded his PhD, Tanenbaum went to work at Bell Laboratories where he did the original studies of single crystal III-V semiconductors. He was asked by William Shockley to head a group to determine if transistors could be made using silicon. Up to that time, all semiconductor technology had employed germanium where the transistor effect had first been discovered. Within a year, Tanenbaum, with the assistance of Ernest Buehler, made the world's first silicon transistor. A few months later, Gordon Teal at Texas Instruments made a similar transistor. Working with Calvin Fuller, Tanenbaum invented the diffused base silicon transistor using solid-state diffusion.

When Shockley left Bell Labs, he invited Tanenbaum to join him to start up a silicon device company funded by Arnold Beckman. After much consideration, Tanenbaum decided to remain at Bell Labs and moved from semiconductors to the broader field of Metallurgy and Materials Science where he led a group including Gene Kunzler who invented high field superconducting magnets. He then moved from the Research Division of Bell Labs to the Electron Device Division where he directed the Laboratory responsible for the development of new devices other than semiconductors, such as solid-state lasers and magnetic memories.

Western Electric recruited Tanenbaum to lead its new Engineering Research Center. He recruited PhD's in the physical sciences and engineering with an interest in applications for the manufacturing floor. He later became Vice President of Engineering for all of Western Electric and then moved from the technical side to become Vice President for Transmission Equipment with responsibility for the several plants that manufactured transmission equipment. Tanenbaum was called back to Bell Labs as Executive Vice President with responsibility for all of development. Then he moved to AT&T Corporate Offices as Senior Vice President of Engineering and Network Services. He later served as President of New Jersey Bell.

In 1980, he was called back to AT&T as Executive Vice President for Administration. During that period, he was much involved in the Federal antitrust case against AT&T that was eventually settled by a Consent Degree that separated AT&T into several independent companies (the "Baby Bells") providing local telephone service and AT&T retaining Western Electric, most of Bell Labs, and the long distance services. As Tanenbaum says, that separation while maintaining quality telephone service was like trying to separate a Boeing 747 into two 737s while in flight. He became Chairman and CEO of AT&T Communications with

responsibility for all long distance service. His final position was CFO and Vice Chairman of the AT&T Board of Directors.

Recognizing that his position, though perhaps more exalted, no longer provided the "fun" he had always sought and found, he prepared to retire. He continued to serve as a Trustee at Johns Hopkins and MIT and on a number of corporate boards. After retirement he consulted for General Motors on their Board's Science Advisory Committee and served as the Vice President of the National Academy of Engineering and a member of the Governing Committee of the National Research Council of the National Academies. He and his wife have always loved classical music and he was a Founding Director of the New Jersey Performing Arts Center while his wife, Charlotte, served on the Board of the New Jersey Symphony. He also served on the Board of the New York Philharmonic.

Throughout the interview Tanenbaum reflects on his enjoyment of his work; his interactions with his colleagues; his retrospective view of the history of transistors, semiconductors, and electronics; his fascination with and insistence on the importance of chemistry; and his long and deep association with AT&T in its various stages.

INTERVIEWERS

David C. Brock is a senior research fellow with the Center for Contemporary History and Policy at the Chemical Heritage Foundation. As a historian of science and technology, he specializes in the history of semiconductor science, technology, and industry; the history of instrumentation; and oral history. Brock has studied the philosophy, sociology, and history of science at Brown University, the University of Edinburgh, and Princeton University.

In the policy arena Brock recently published *Patterning the World: The Rise of Chemically Amplified Photoresists*, a white-paper case study for the Center's Studies in Materials Innovation. With Hyungsub Choi he is preparing an analysis of semiconductor technology roadmapping, having presented preliminary results at the 2009 meeting of the Industry Studies Association.

Christophe Lécuyer is a graduate of the École Normale Supérieure in Paris, and he received a Ph.D. in history from Stanford University. He was a fellow of the Dibner Institute for the History of Science and Technology and has taught at the Massachusetts Institute of Technology, Stanford University, and the University of Virginia. Before becoming a senior research fellow at CHF, Lécuyer was the program manager of the electronic materials department. He has published widely on the history of electronics, engineering education, and medical and scientific instruments, and is the author of *Making Silicon Valley: Innovation and the Growth of High Tech, 1930–1970* (2005).

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INTERVIEWER: **David C. Brock and Christophe Lécuyer**

LOCATION: **Bell Telephone Laboratories, Inc.**
Murray Hill, New Jersey

DATE: **3 May 2004**

BROCK: This is an oral history interview with Morris Tanenbaum on May 3, 2004 at Bell Labs, Murray Hill, [New Jersey], and David Brock and Christophe Lécuyer are interviewing. So, to begin at the beginning, we know that you were born in 1928 in Huntington, West Virginia. And…interesting to know a little bit about your family background, what brought people to West Virginia, and about your parents. Tell us a little bit about that.

TANENBAUM: Fine. My father and mother were immigrants. My father was born in Kobryn, which was [in] Russia at the time (I guess it still is [in] Russia […]). My mother was born in what was then Poland, maybe fifty miles away in Brest—Brest-Litovsk, I guess I should say. Famous for a treaty. [laughter]

They met, I was told, because my father's older brother, Leo [Tannenbaum]—my father's name was Ruben Simon Tanenbaum—his other brother, Leo, was dating my mother, and I don't know any of the details beyond that. [laughter] But my mother and father immigrated here. My father told me later, he was about to be inducted into the Russian Army, and he was…he understood that was not a good place for a young Jewish boy. So he—and I've met many other people who have the same story—they left. [My parents] could not come here directly because of the immigration restrictions. So they went first to Buenos Aires, Argentina. My older sister, Genevieve [Tanenbaum] was born just before they left. So she was a babe in arms at the time. They spent a year in Buenos Aires and then got clearance to come here. Now, my father's older brother, Leo, had immigrated earlier than that, perhaps when he became eligible [for the draft], but I don't know that. He was in New York, and I think it was through his agency, actually, that my mother [Mollie Tanenbaum] and father were able to come here.

BROCK: This was the middle 1920s…?

TANENBAUM: It was, I think…actually, they arrived in 1922.

BROCK: Okay.

TANENBAUM: I know that date because my wife, who has a good friend [Darlene Eckel] who was doing a study for her son and looking at archives for family history, [she and her family] went to Bayonne, New Jersey […], which was a [Naval] registry for a while. I think that's been [moved]. My wife had nothing better to do, so she started going through the records and she actually found the [register of the ship] on which my parents arrived in New York. So that's how I know that date.

My father had been in the hardware business in Kobryn when they left. When they came here, he wasn't sure what he was going to do. His older brother had a small delicatessen in Queens…no, no, in the Bronx, at the time. Then, apparently, my father heard from another friend—[with] a family [name of] Rubenstein—who was a baker and had a bakery in Huntington, West Virginia. I have no idea how [Rubenstein] got there, but he told my father that "There's no delicatessen in this town. There were hardware stores, but there's no delicatessen." He thought perhaps that that would be successful. So my father and mother moved there, and that's where I was born.

BROCK: Right.

TANENBAUM: I was born upstairs from their first store, and then later my father…[the store] was successful enough for my father to buy a building in the center of town. We moved there, [above that store], and I spent my first [few] years [there]; I don't remember the first store at all. I have a photograph <**T: 05 min**> of it, but I don't remember it all. So my real memories begin, essentially, living upstairs above his second store.

BROCK: So, their intuition must have been correct, that it was successful, ultimately, though?

TANENBAUM: It was.

BROCK: Yes.

TANENBAUM: It was successful. That's right. We lived there for a while. Actually, I started elementary school [Buffington Elementary School] in a school located just a few blocks away from the store. Then, my sister, [Annette Tanenbaum, arrived…] (there were a total of three of us in the family). My younger sister was born, […either] while we were living there or shortly thereafter, but I think it was at that point in time that my mother and father decided that they needed a house.

BROCK: I see.

TANENBAUM: They rented a house over in what was called the "south side" of town, which was the principal residential area. We moved there. It was in the same block as an elementary school, which I guess is one reason they chose it. I transferred to Miller School at that time, and went up through the sixth grade in Miller School. It was just about at that time that my family decided they needed a bigger house, so they rented another house, maybe a half a dozen blocks away, which was closer to the junior high school [Cammack Junior High School]. That's where I went, and we lived there four or five years. Then, finally, they bought a home a few blocks away from there.

BROCK: Could you tell us a little bit about what sort of a place Huntington was like during the 1930s growing up?

TANENBAUM: Yes. […] Of course it was during the Depression period. But Huntington was the largest city in West Virginia. The population was about eighty thousand. The capitol was Charleston, which was about fifty miles [east]. My recollection is that its population was about sixty thousand. There were, I think, to the best of my knowledge, there were three principal industries there.

Huntington was the terminus of the early Chesapeake and Ohio Railroad, which was founded by Collis P. Huntington (that's why it was named…). It was laid out by an engineer. So all the…it was on a perfectly rectangular grid and all the streets were numbered one way, and all the avenues ran perpendicular. You could tell from the street number just exactly where the location was. So when I first moved out of [the city to Baltimore], I could hardly find my way around […].

But the three principal industries: American Car and Foundry had a large repair center there for railroad cars; International Nickel, Huntington Alloys Division, was also there, and a major industry; and Owens Illinois had a glass factory there where they made […] bottles. I think those were the principal industries I was aware of when I grew up. There was still a lot of railroad people there, because it was still [a] center for the railroads. A lot of the coal from southern West Virginia, where most of the coal mining was, moved through Huntington.

BROCK: Right.

TANENBAUM: […] Kids liked to go out on the railroad tracks. Parents hated it, but the kids liked to watch the big trains come through. It was a wonderful place to grow up as a kid. I really liked it. I think the schools were good. I don't think I can go back and live there these days.

I think Huntington has…I still have one friend [Bob Levy] that I correspond with in Huntington, but the town has just changed very little. I went back there about fifteen years or so ago, and the houses that I lived in are still there, and they look the same. Where my father had his store is no longer there, that was taken over by the branch of a furniture store nearby. But the town had changed very little.

BROCK: Having their own business, I would imagine that running the store really occupied a lot of both your father and your mother's time <**T: 10 min**>.

TANENBAUM: Yes.

BROCK: Is that right?

TANENBAUM: That's right. We had a live-in…I don't know what to call her…second mother, I guess I'll call her. She [Josie Hamby] lived with us and did the cooking, and kept the house, and what have you, and sort of watched over the kids. She was from the hills of Tennessee, and really [a] very wonderful person. I remember her very fondly. She was an amateur artist, too. She painted landscapes and things in a rather primitive style, but I still have some of her pictures. She did great pictures.

BROCK: The elementary and middle schools that you mentioned, those were both public…

TANENBAUM: Yes.

BROCK: …schools?

TANENBAUM: Yes.

BROCK: Yes. Can you tell us a little bit more about your education there?

TANENBAUM: I can tell you what I remember. I was a pretty good student. I was double promoted once. So, actually, when I started college, I was just before my seventeenth birthday. I wouldn't say I had inspirational science teachers, except in middle school, where we had kind of general science. The fellow who was a good lecturer and liked explosions and things of that sort, because it got kids' attention, Madsen was his name, Mr. Madsen.

Then…but I had chemistry and physics in high school. The usual mathematics. And Latin was a language that was still not so much…well, it was required if you were in that so-called college track. I guess that would be the only language I had. No, no, I had Spanish, too. I had a wonderful Spanish teacher. We started speaking nothing but Spanish after about the first half a dozen weeks. I could carry on a simple conversation in Spanish by the time we finished. What I was really thinking of…I didn't have any of the languages I really needed for my Ph.D.

BROCK: Right. [laughter]

TANENBAUM: So I had to learn those separately.

BROCK: Well, you mentioned Mr. Madsen. Could you tell us a little bit more about the development of your interest in science, math, [and] things technical?

TANENBAUM: It just sort of came, I guess I'd have to say. I got interested in chemistry, but before I had any Chemistry. I was given […] one of these little chemical sets. I had a room up on the third floor of our second rented house, where I learned how to take paint off of tables, accidentally, and things of that sort, and learned to make gunpowder, which my mother didn't think was such a good idea. I never really tried to explode it, but I just liked to see it burn. You know, make hydrogen gas, and ignite that, and those kinds of dangerous things that I probably shouldn't have done.

My mother brought me to the World's Fair in…the 1939 World's Fair in New York. It was really my first trip out of West Virginia. I was really very intrigued by what I saw there. So I think that, outside of my natural curiosity, is the principal thing that really sort of got me interested in […] what I wanted to do.

BROCK: Just that image of science and technology in the future at the World's Fair.

TANENBAUM: Right, right.

BROCK: Yes.

TANENBAUM: All the exhibits and the demonstrations and what have you. That's really …

LÉCUYER: Were you involved with amateur radios at any point?

TANENBAUM: I did. I actually had a friend who was a little more interested in the physical things.

LÉCUYER: I see.

TANENBAUM: And we did make radios, you know winding wires on an oatmeal box. I never really understood what the crystal <**T: 15 min**> was, but I knew you had to fool around with it before you got reception. But I never…I understood very little about the physics of it.

BROCK: Could you maybe tell us a little bit more beyond your home laboratory, and your questionable activities upstairs, about the range of your other activities as a young man?

TANENBAUM: […] Actually, I spent a good deal of time in my father's store.

BROCK: Okay.

TANENBAUM: I was expected, after school, to go there and dust shelves and rearrange things. So my extracurricular activities really didn't start until I got into high school. I was interested in theater there and […] I was in the school play. Public speaking…I learned a bit of public speaking. What else? I don't think there was a mathematics club. I don't think so. My recollection of the extracurricular activities is that they were rather limited back in…when was this? 1940s, I guess, the early 1940s.

BROCK: Well, clearly someone was encouraging your natural curiosity by giving you a chemistry set. Was learning a big education…was learning a big value in your household?

TANENBAUM: Oh, it was perfectly clear to me from the first I could remember that I was expected to go to college. My older sister went to college, Ohio State, [and] majored in statistics, I think, in mathematics and statistics. She was a very good student too, so she was kind of my competition when I followed her in school. They remembered her. So there was just no question about the fact that education was very important.

BROCK: Right. In addition to that value placed on education, was there…what was the emphasis on religion or art or science in the household? Or literature? Could you maybe just speak to that?

TANENBAUM: Sure. So my father…my grandfather, whom I never met—and, in fact, all of the family was essentially wiped out during the Holocaust—but my grandfather was [a] rabbi. My father was brought up in a very, very religious home. We belonged—the family belonged…there were two congregations in town, the Reformed Congregation, and a Conservative one. We belonged to the Conservative one. My mother kept a kosher home. [The congregation] was relaxed Conservative, I guess I would say. I went to the synagogue with my father. I went through a Hebrew school there, and had a bar mitzvah.

But I must say I was never really convinced that religion was going to be an important part in my life. I think I…I wanted a little more demonstration that there was a substance other than belief involved with it. In my later life, when my children—I have two children, [a] son and [a] daughter—we belonged to a Reformed Congregation. They went through [Hebrew] school, and my son was bar mitzvahed and I don't think he's been inside a synagogue since. My daughter, on the other hand, still is a member of a congregation where she lives. She is the most religiously inclined of the family.

BROCK: It sounds from our earlier conversation that your family, at least, escaped a lot of the worst of the Depression Era. Can you tell us a little bit what that experience was like living through that?

TANENBAUM: Well, I never really knew there was a Depression…

Let's see. I guess the Depression ended, really, with World War II. And 1941 <**T: 20 min**>, I was thirteen years old. So I wasn't very conscious of that, except from […] occasional conversations I'd hear my parents…I remember my father comment that, "During times like this, it's nice to have a food store. You always had something to eat. People need food."

There was a calamity with my father and his business. There was a major flood in Huntington in 19…it was either '37 or '39 [The Great Flood of 1937] (I've forgotten). His store was under ten or twelve feet of water at that point. He never really recovered from that. He continued the business, but he never really was able to recover. My family had decided to move out of…my sister, [Genevieve], got married and left. My mother and father had decided to move out. This would have been back around 1949, 1950. But he became ill and died before they…just before they moved.

BROCK: Oh, dear.

TANENBAUM: And so, my mother did move. If you want a little romantic [side-story], after probably five or six years, she married my father's older brother, [Leo], whose wife was killed [in an automobile accident] several years before…

BROCK: She had originally…

TANENBAUM: Yes. Yes, see.

LÉCUYER: Oh, wow.

BROCK: And where did they live?

TANENBAUM: Well, they went to Baltimore.

BROCK: Okay.

TANENBAUM: My sister's husband [Irvin Small] was from Baltimore. So, she had moved there.

BROCK: I see.

TANENBAUM: I ended up going to Johns Hopkins for my undergraduate degree. My mother really wanted me…my father wanted me to be a lawyer; my mother wanted me to be a doctor, an MD. I wanted to be a chemist. I can remember when I first really told my parents that I really wanted to study chemistry, that I wanted to be a chemist. This was back, well, before World War II. I can still remember them saying, "That's nice, but how are you going to make a living?" They knew chemists and chemical engineers that were out of work and having difficulties.

But that's how they got to Baltimore. In fact, one of the reasons I went to Johns Hopkins was that they had a reputation in chemistry: Remsen Hall [named after Ira Remsen].

BROCK: Right, yes.

TANENBAUM: And my mother thought I surely would become interested in medicine, if I went to Johns Hopkins.

BROCK: Yes.

TANENBAUM: The fact that my sister was there, they felt more comfortable about sending their sixteen-year old to college in Baltimore.

BROCK: So…

LÉCUYER: That was in 1944, wasn't it, or…?

TANENBAUM: […] I started in '45.

LÉCUYER: Okay, thank you.

TANENBAUM: Actually, my seventeenth birthday was just about three months after the European victory [V-E Day] and the draft was discontinued. Otherwise, I would have gone directly into the Armed Forces. Because of that, because I just missed it, I usually found afterwards, throughout my career, that for the most part I was associating with people that were three or four years older than I was because they had spent time in the service.

BROCK: Right. What was…so the war really coincided with your high school experience.

TANENBAUM: Right.

BROCK: Could you tell us a little bit about what sort of cast that gave to going to high school in that era, or was it something in the paper?

TANENBAUM: Nothing I noticed.

BROCK: Yes.

TANENBAUM: Yes, nothing I noticed.

BROCK: And your experience in the chemistry classroom, I guess, was confirmatory of your desire to…?

TANENBAUM: Yes, although, I think the interesting thing is I did not have an inspired chemistry teacher, but I was sufficiently interested and it didn't make much difference. The physics teacher was a much more interesting teacher. We got involved in building a Tesla coil to make lightning. That was an after school activity that I occasionally was able to participate in. So that <**T: 25 min**> certainly helped solidify my interest in science at large.

BROCK: And you continued to find just a natural aptitude, if you will, to the technical, mathematical…?

TANENBAUM: Yes.

BROCK: What was it…? Can you recall what was it that really captured your imagination in chemistry?

TANENBAUM: No single event, no single thing. I began to understand the different parts of chemistry when I was in college, and decided that I really liked physical chemistry a lot better than organic chemistry. It had more…I guess I felt [it had] more logical structure to it. Organic chemistry, at that time, was still developing. A lot of memory, a lot memory. And I preferred to have to remember only a few things, and then be able to develop the rest of it.

BROCK: In thinking about your high school peer group, was it an unusual thing for you to be going off to Johns Hopkins? Or, what were other…

TANENBAUM: No, most…

BROCK: …peers doing?

TANENBAUM: No, most of my friends went to college. I had a lot of friends that weren't Jewish. I mean, Jewish kids are sort of the…it's unusual if they aren't pushed to college. But I guess I was sort of a…I had no idea what fraction of my class went to college. But I guess you tended to sort of segregate yourself with people [with] similar interests, and most of the people I knew were heading to college.

BROCK: Did you explore anywhere other than Johns Hopkins? Or did you concentrate on that?

TANENBAUM: No. I applied to a number of schools; I don't remember all of them. I applied to MIT [Massachusetts Institute of Technology]; I was put on the waiting list. And I decided to go to Hopkins before I knew whether I was going to be admitted there. I applied, I think, to Ohio State where my sister had gone to college. I applied to Vanderbilt, because they offered me a scholarship. But I decided to go to Hopkins. I don't recall […] where else I applied. It wasn't that difficult getting in when I applied. It got more difficult after the first wave of veterans arrived. But when I arrived at Hopkins, the total [undergraduate] student body was about…my recollection is about four hundred students.

BROCK: Oh, God.

TANENBAUM: Then in February, it went up to about fourteen hundred students. The first wave of the veterans arrived. I was playing varsity basketball at Hopkins the first semester, and I couldn't even make junior varsity the second semester.

BROCK: Could you tell us a little bit more about what the university was like when you were there, and from, I guess, '45 to '49, was it?

TANENBAUM: Yes. Well, it was going through a substantial growth period in terms of the student body. The student body was changing a lot. I got involved in a lot more extracurricular activities at Hopkins. I ended up being the business manager, for some reason, of most of the things I got involved with, [like] the theater group, which was called the Barnstormers. Then we had a group called the Cotillion Board, which arranged all the school events. We brought the big bands and what have you. And, as I say, I ended up being the business manager in most of those things. The chemical fraternity: Phi…what is it? Phi…

BROCK: Alpha Chi Sigma?

TANENBAUM: No, no, no.

BROCK: No.

TANENBAUM: Isn't that terrible…I was president of that [society, Phi Lamda Upsilon].

[…] Anyway….

BROCK: Do you think that business manager role was from your tutelage in your parents' store? Or…?

TANENBAUM: No, I don't know why, <**T: 30 min**> to tell you the truth.

BROCK: Had you ever considered going into business for yourself at that time, or just focus on the chemistry?

TANENBAUM: No, I figured I would go to work in an industrial chemical lab, DuPont [E.I. DuPont de Nemours and Company], or something like that.

BROCK: And so it was during your undergraduate studies that you became focused to physical chemistry.

TANENBAUM: Right.

BROCK: Could you tell us a little bit about your experience with your chemical studies as an undergraduate?

[END OF AUDIO, FILE 1.1]

TANENBAUM: […] You were asking me what I remembered about chemistry…

BROCK: Yes, sure.

TANENBAUM: …at Hopkins. I was thinking about it as [we were] walking. Well, I remember my freshman chemistry teacher in particular, John F. Baxter, [Jr.], who was really an excellent teacher, and hard, and tough as nails. In fact, at our very first class—and this was in the big lecture hall at Remsen Hall—our first class he said, "I want you to know that I have a very high regard for professional medicine." […] He said, "Although, you probably haven't admitted it, my experience tells me that more than half of this class are pre-meds." At this time, by the way, chemistry was the preferred undergraduate specialty to get into medical school. He said, "So I want you to know, I grade on a curve. If this class is like most of my classes, half of you will not finish."

BROCK: Oh, my God. [laughter]

TANENBAUM: And he was right. He was right. Among the students the saying was, you know John F. Baxter, that F stands for "Ferrous." We had a quiz on Monday morning every week. It was about a ten or fifteen minute quiz, ten questions. My objective…and then, posted on the next morning was how everybody did. So you got to see…you saw what you did, and you saw how everybody else did.

LÉCUYER: Thank goodness.

TANENBAUM: Yes. My objective in life was to get a hundred on one of those quizzes. […] Never made it. I thought I made it at one time, and I got a ninety-nine. On one of the equations, it was you had to answer the equation on hydronium ion. I had not put in a plus [sign in H_3O^+]

LÉCUYER: Okay.

BROCK: Yes.

TANENBAUM: Yes. So I went into argue with him about that, and said, "You know that I know the hydronium ion has a plus. You know that I understand this. I don't understand why you marked me out." He said, "Does hydronium ion have a plus?" I said, "Yes." He says, "Is this correct." I said, "No." He said, "Case dismissed." So things like that stick in the mind.

The other courses…I really didn't like organic chemistry at all. I don't know how I did. I probably…I may have gotten an A. I got an A in most of my science and labs. But I may have gotten a B there, too. I don't think so, but I might have. It wouldn't have bothered me, put it that way. And physical chemistry was tough, but I had Professor [Walter A.] Patrick. One of the things I remember from him, he had done some very basic work early in his career on silica gel, got patents on silica gel. It was known that he…I don't think he had any graduate students, and he didn't seem to work very hard. I remember him telling his class, "The secret to success is to do something important while you're young, and then clip coupons the rest of [your] life." [laughter]

But other than that, you know there are little things like that, I guess, that pop up in my memory now and then. But I was very satisfied with chemistry as a choice. I got somewhat more interested in physics. We had a course on atomic physics, and so that was my first contact with quantum mechanics. Math was taught by…gosh, I can't think of his name [Francis Murnaghan]. Any rate, he was an Irishman, with one of the really good textbooks on applied mathematics—it wasn't really applied mathematics <**T: 05 min**>—which he dedicated to the glory of God and the future of Ireland. But that…and the course, I never really learned how to solve [integral] equations very well, because the course was not on how you solve them, but how you formulate them. I've always found that was very, very helpful, and, particularly, these days you don't need to know how to solve [integral] equations: the computer will do it for you.

BROCK: Was there quite a bit of laboratory work involved with your chemistry education?

TANENBAUM: I think pretty standard, pretty much standard thing. Probably the most important thing that happened to me there was you took analytical chemistry, and the professor there was Clark [E.] Bricker. He had gotten his PhD at Princeton. I don't know how he got to Hopkins. He was assistant professor at Hopkins, but he was offered a tenure track position at Princeton just at the time that I was graduating. Graduating in '49, that was a kind of down period in the economy, and I did a little interviewing for jobs, but I didn't see anything that really turned me on. So, Professor Bricker suggested I go to graduate school. I had never even…I didn't know what graduate school was. He explained it to me, and he said, "I can get you a fellowship,"—not a fellowship, but assistantship—"so that it'll pay your way. It won't give you a lot to live on, but it'll pay your way." And that was…I had just gotten married…no, no, I had gotten married, I'm sorry, a year after I went to graduate school.

But we were…I was engaged. And since I could pay my own way there, it sounded pretty good to me. So that got me to Princeton, and got me into graduate school. Otherwise, I think I probably would have taken a job someplace and my career path would have been all together different.

BROCK: What was Bricker's research in analytical chemistry? Spectroscopy?

TANENBAUM: I think it was rather broad. What I did as a research assistant was spectroscopy. Yes. He had…let's see if I can remember what the chemical was. He was consulting with a chemical firm not far away. The problem was really quantitative detection of the free formaldehyde, but I've forgotten just exactly what it was. We published a paper on it, actually.[1] So…

BROCK: Oh, I think this was the formaldehyde…. Isn't that what you published on?

TANENBAUM: This was…

BROCK: This was earlier.

TANENBAUM: Yes. It was…I'm trying to remember. I think in order…I really have to go back and read that paper. But, it was a colorimetric analysis on an old—I think the first generation of the Beckman spectro[photometer]. I think it may have been a reaction which produced free formaldehyde which then was the detectable. Unfortunately, the color had a very short half-life. By short, I mean a matter of several minutes or so. So you had to time everything very carefully. But my interest really became more and more strongly focused towards physical chemistry.

BROCK: And he…and this conversation with Bricker occurred because you became acquainted with him in his analytical chemistry course?

TANENBAUM: Yes, right.

BROCK: I guess at this time, sort of new instrumentation incorporating electronics, like the Beckman spectrophotometer were just sort of coming into the analytical lab…?

TANENBAUM: That's right.

BROCK: Did that…?

TANENBAUM: That's right. It was a pH meter and a spectro[photo]meter, were the only two things that had come inside <**T: 10 min**> [the lab].

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[1] Morris Tanenbaum and Clark E. Bricker, "Microdetermination of Free Formaldehyde," *Analytical Chemistry* 23(2) (1951): 354-7.

BROCK: Was there any opportunity at Hopkins to take a course in industrial chemistry or chemical engineering that you…?

TANENBAUM: There may have been an opportunity, but I didn't do it. In fact, it didn't even cross my mind. I filled my course [load] with science and math, and whatever was required in the humanities. My languages…I did study German there, but that was one of the languages. They used to have to have two languages to get your PhD. I just was wondering what I should take as a second language, once it became clear to me [that] I needed two languages. I decided Russian was probably [good], because Russia looked like it was really, really, really emerging. So I took a night course that [taught Russian]. After a year of that, I decided that if I was really going to rely on Russian for my second language, I was never going to get a PhD. So I ended up doing French, which was pretty much self-taught.

LÉCUYER: Right. So, if you could go back to the chemistry, about [research and teaching].

TANENBAUM: It is hard for me to say. If there were opportunities to do research as an undergraduate, I wasn't aware of it. Things are very different today.

BROCK: Again, the question of peer group, with your fellow chemistry students, I suppose the majority of whom were pre-medical, you know, destined for a medical degree…. For those who weren't, can you tell us a little bit about that coterie that you went through with? Do you keep in touch with any of them? Do you know what their experience has been like?

TANENBAUM: I'd have to say, really, no. There was only one fellow who I ran into again. In fact, we ended up living just about a block away from each other—when, later on, I was living back outside of Princeton—who had got a job with American Cyanamid and was working with their agriculture division. I guess it was nearby. But, no, really no. I lived in a fraternity house, and that's really where most of my social life revolved, and most of my friends were, and most of my fraternity brothers were pre-meds.

BROCK: […] Oh. Well, you did mention that you became engaged at the close of your undergraduate experience. I was wondering if you can tell us a little bit about your then future wife, and…

TANENBAUM: Love to…

BROCK: …who she was, and…yes?

TANENBAUM: Sure. I met Charlotte (her last name was Silver)…the time I could really pin down was in my sophomore year and at the fraternity house. She was the date of one my fraternity brothers who had lived in town. Half of the fraternity lived in town. […] Although Hopkins was [principally] a residential school, I would guess…I don't know what the fraction was, but it had to be at least a third of students lived in the vicinity of Baltimore. A lot of them lived at home. And that's how you met most of the girls, because Hopkins was not co-ed at that time.

BROCK: Oh, right.

TANENBAUM: And that's why, if you wanted any social life at all, you went into a fraternity house. I think both of us think that we might have at least been introduced to each other a year earlier […] during my freshman year, when I was at some kind of a party or some sort of an affair off campus. But at any rate, that's when I really got to, really, really got to know her. We started going steady pretty quickly, and that took care of my social life for most of my college career.

BROCK: Was she also in school at that time?

TANENBAUM: She was going to the night school at Hopkins <**T: 15 min**>. It was called McCoy College at that time. But she was working as a secretary, and she never got a degree, an undergraduate degree. But she's always been very interested in history. Whenever I have any questions about anything relating to history, I go to Charlotte. She continued from time to time taking courses. She got very interested in China for a while, and we were always living somewhere close enough to a university where she could find night courses.

We were both interested in classical music. In fact, when I first started courting her, in order to—before we had become so close—I figured one way to assure a steady string of dates was I bought two subscriptions to the Baltimore Symphony Orchestra, and that worked very well.

BROCK: Yes, I can imagine. Is she a Baltimore native?

TANENBAUM: Yes.

BROCK: Yes.

TANENBAUM: Yes. She was born and raised in Baltimore. Her father was also a Russian immigrant, but her mother was born in Baltimore, too.

BROCK: And how did she react to the proposition of going…of you going up to Princeton? I guess you had discussions about transplanting…?

TANENBAUM: Oh, yes, yes. We did. I pointed out that it was on the main line of the Pennsylvania [Railroad] and I would make trips back every two or three weeks during that period until I got sick. They thought it was mononucleosis, but they could never really pin it down. I had originally had kind of an understanding with her that when I finished graduate school we'd get married. But after that first year, it was clear that I wasn't going…I was not going to continue commuting like that, back and forth. So, we got married. She really, financially, made our life much more…she got a job at RCA [Radio Corporation of America], in there as a secretary in the patent department.

BROCK: At the Sarnoff lab.

TANENBAUM: Yes, at the Sarnoff lab, and so…

BROCK: Great.

TANENBAUM: …Charlotte and RCA made our living very comfortable for the last two years in graduate school. In fact, I had fellowships at that point in time, which paid well, and she had a good salary. When I finished graduate school, and went to work, I started off with what was really a pretty good salary at that time. I think it was five hundred…five-fifty a month. It became clear to us that we actually did a little bit better while I was a student. But she got another job and worked until our first child was born.

BROCK: Let's see. I have…

TANENBAUM: I did work. While I was an undergrad—I said I wasn't involved in research—but in the summer times I got jobs where I was. Now the first one was at, what was then, Davison Chemical [Company]. They were the large producers of silica gel. Professor Patrick helped me get a job there. What I was doing there was…they were trying to develop low-density silica gel for use as flattening agents in varnishes. So I was sitting over a microscope doing particle counts and particle size measurements, and then stirring it up together and seeing how it looked afterwards with a reflectometer. I didn't like that very much.

So the next two years, I got jobs at Edgewood—it was then called Edgewood Arsenals; it's now part of the Aberdeen Proving Grounds [in Maryland]—…

BROCK: Okay, yes.

TANENBAUM: …which was their chemical research activities, and worked on mustard gas and nerve gas. When I look back on that and realize they should never have let me do that…

BROCK: Why […]?

TANENBAUM: Because it's very dangerous.

BROCK: Okay, yes.

TANENBAUM: Very dangerous, and nerve gas in particular. The first thing they do is teach you…show you where the hypodermic, the atropine, is. If you ever get any contact, you take that and jam it in, and unload it into a fleshy part of your body. We were trying to purify the agents, by a <**T: 20 min**> fractional, by a crystallization…fractional crystallization [at] low temperature. Unfortunately, it didn't crystallize very well, but I was working with high concentration nerve gas.

BROCK: And you worked there for two summers.

TANENBAUM: Two summers, right.

BROCK: Now was there a connection between the Hopkins Chemistry Department and the Arsenal?

TANENBAUM: You know, I don't know how I got that job. I don't know how I heard about it. It must have been advertised someplace. I don't recall any particular individual setting that up.

BROCK: And that was a [U.S.] Army facility, huh?

TANENBAUM: Yes.

BROCK: Yes.

TANENBAUM: Yes. Yes, outside of Baltimore, half an hour or so ride. Got in a carpool.

BROCK: And what did you make of working in that government context? Did it…?

TANENBAUM: It seemed okay to me.

BROCK: Yes.

TANENBAUM: Yes. I thought they were doing interesting work. That was one of the places I thought about when I…that I might be interested in.

LÉCUYER: That was your first experience with research?

TANENBAUM: Really, yes, that's right. That's right.

BROCK: Well, maybe we could move on to asking you to tell us a little bit about Princeton as you found it…in 1949, I guess, is when you made the move…

TANENBAUM: Right.

BROCK: …and the university in particular and in particular the chemistry department in those days.

TANENBAUM: Well, as a graduate student at Princeton, the chemistry department was the university.

BROCK: Right, okay. Yes. That's good.

TANENBAUM: And when I first went there, I was research assistant for Clark Bricker. And the lab I worked in had another graduate student, Charlie Riley, I guess. Yes, it was Charlie Riley, who was an electronic whiz. He was an instrument developer. I recall his thesis consisted of about half a dozen published papers bound together and that was his thesis, a very capable guy. He later went, I think, to University of North Carolina, Chapel Hill. I would occasionally see his name in *Chemical & Engineering News*. Then I lost track of him […].

But any rate, I got interested, a little interested, in electronics then. In particular, he helped me put together a stereo… it wasn't stereo then, but it was a good amplifier that you could build from a kit, and put speakers that you could buy and build cabinets for it. With our classical music interest, that worked very well.

So I did…all these things, tended, really, to push me into physical science, in that direction. Let's see. How did I get to that?

BROCK: Well…

TANENBAUM: You…. Yes, you were asking me about Princeton…

BROCK: The department, yes.

TANENBAUM: Right.

BROCK: So how big was…you initially you came into Bricker's group, I guess…

TANENBAUM: I think I was in Bricker's Group…

BROCK: Oh, okay. Oh, right.

TANENBAUM: …because he was just arriving, too.

BROCK: Yes.

TANENBAUM: No, no. Well, I guess Charlie Riley was also a part of Bricker's group. That's right.

BROCK: Okay. Was it in the context of coursework that you were doing, where you…did you drift out of Bricker's group into a physical chemistry group, then? How did that…?

TANENBAUM: Yes. Well, I…that was kind of my plan anyway, was physical chemistry, not analytical chemistry. Bricker knew that as well. At that time at Princeton they had what, in retrospect, was, I think, just an excellent way to handle graduate students. The dean of the graduate school was Hugh Stott Taylor, who was…he was a physical chemist, catalycist, I guess was really his specialty. First of all, he didn't think graduate students ought to be married <**T: 25 min**>, that graduate students don't have time to be married. Of course, when veterans arrived, he had a lot of trouble with that, and I decided to get married anyway.

But the important thing was he thought you should get your PhD in a maximum of three years. He said…what you were told was, "You could stay here as long as you want, but you won't get any support from the university after three years." Okay?

BROCK: Okay.

TANENBAUM: And the typical course of action was two years of…you took courses, principally courses in your first [year] and some in your second year. Then you took your general examination. If you pass that, you automatically got a master's degree. Then we'd work full time in research for your third. You actually start your [thesis] research in your second year.

Well, I had decided that I really wanted to have two [full] years of research, so I wanted to take my generals at the end of my first year. We had just gotten married. We were lucky enough: I had to camp on the door of a housing department there; we got a nice apartment in an old apartment house that the university owned that was filled with more senior graduate students as well as faculty […].

I crammed all my coursework into my first year and took my generals at the end of my first year. My wife, Charlotte, showed that she had…she had to show that she was fond of me to let me do that, because we lived in the same apartment, but—and we slept in the [same] bed—we didn't see much of each other…

BROCK: Yes.

TANENBAUM: …other than that. I was, at the same time…you picked your own professors as long as he was willing to accept you. I went to work for Walter [J.] Kauzmann as my professor of physical chemistry, who had a very good reputation. He was about the only person at the department who was really involved in quantum chemistry at that time. He wrote, I think, one of the early textbooks on quantum chemistry[2] and we used his notes, his mimeographed notes—it was before xerography—for his book in his course.

He had been a student of Henry Eyring's. So he was very interested in absolute rate. He had a graduate student working for him, whose name will come to me soon. Larry was his first name. Kauzmann, […] after he got his degree, [went] on to Westinghouse [Electric], and was working in metals. He got very interested in the fact that the single crystals of metals are, […] mechanically, extraordinarily weak. No one could understand why. This was before…although, I guess, people, particularly [Sir Nevill F.] Mott in England, had started thinking about dislocations.

[…Kauzmann's] student, the graduate [student] who preceded me, had built an apparatus to study the kinetics of mechanical deformation, [known as] "creep." [He grew] single crystals [of zinc and] then put them under [low] stress and [measured their] deformation […] with time [and temperature. His name was] Slifkin, Larry Slifkin [Lawrence M. Slifkin].

BROCK: Yes.

TANENBAUM: Larry had built the apparatus and started to take some measurements and had enough to write a thesis. I inherited his equipment and worked to increase its sensitivity and to improve the quality of the crystals [using] zinc metal. A well-grown, zinc single crystal [was] the size [and shape] of [a] pencil. Going by the Bridgman Method, if you were to take [such a crystal] and lift it up [horizontally] by its end, it would [slowly bend under its own weight]. You couldn't make it straight. In order to keep it straight, you had to hold it [vertically] <**T: 30 min**>.

BROCK: Oh, yes, right.

[2] Walter Kauzmann, *Quantum Chemistry: An Introduction* (New York: Academic Press, 1957).

TANENBAUM: It would…the force of gravity would just make it deform [very slowly]. So the idea was to grow crystals as perfect as you could, then […] measure […] their deformation with a rather, really simple [homemade, mechanical extensometer].

[END OF AUDIO, FILE 1.2]

TANENBAUM: [As the crystal elongated, a mirror in the extensometer rotated. Using a telescope, we could observe a scale with millimeter markings some ten meters away and measure crystal elongations in the micron range. I sat at the telescope for hours on end taking readings and calculating extension rates with a circular slide rule. Today you would use an electronic extensometer and a computer would record data and calculate rates while you were having lunch.]

So you look at that as a function, the function in temperature, and then you […calculate] the activation energy […]. What you found was, what I found was, that the activation energy displayed a very, very large entropy factor, a free energy factor that was [of] several thousand calories, but [with] a very, very large entropy factor, which told you that [the mechanism of] deformation had to be something that […] required great coordination in the crystal, [something like a dislocation]. There were theories that it could be simply the migration of vacancies in as crystals formed, but [my measurements strongly contradicted these theories]. So that was the basis of my thesis.

BROCK: And how are you growing these crystals, because that was one of the…

TANENBAUM: Yes, by the…

BROCK: …they were very high quality.

TANENBAUM: By the Bridgman Method. You bought the purest zinc you could get, and you get quite pure zinc, five-nines pure zinc. Then I'd [distill] it a few times [in high vacuum] just to try to clean it up even more. Then you encapsulate it in a silica tube, and under high vacuum, 10^{-6} millimeter anyway. I had to build the crystal growing apparatus, […] with McCloud gauges, and what have you, back in the vacuum setup [for the distillation]. Then, in a furnace, [above the melting point of zinc], then you very, very slowly either pulled the silica tube out or lower the furnace.

BROCK: I see.

TANENBAUM: And if you [form] the silica tube [with] a fine tip at one end, the far end, with the hope that you will get nucleation of only a single grain, a single crystal of [distillation]…

BROCK: Okay.

TANENBAUM: …and then do it slowly so that that grain grows and fills the [tube]. It turns out [that] zinc is very easy to grow in single crystal form [that way].

BROCK: Okay. So it must have been very quickly that you moved from this work that you were doing with Bricker on the spectrometric measurements of the formaldehyde, free formaldehyde, to the crystal work.

TANENBAUM: Yes. [I made that transition at] the beginning of my second year.

BROCK: Okay.

TANENBAUM: So I had two years essentially of…not to have to worry about anything, except research.

BROCK: And what was your…what were your general exams like after compressing all your coursework into one year?

TANENBAUM: Yes. Well, the general exams <**T: 05 min**> […consisted of] four half-days in analytical, organic, [and] inorganic [chemistry, plus…] a full day exam in your specialty, which in my case was [physical chemistry]. You did a lot of cramming. You know the principal reason for the coursework was that that gave you an idea of what the [general] exams [were] going to be about. And they were straightforward. I did well in them.

[…] The physical chemistry exam, I think, was, I don't know, [about] a dozen questions […]. You had to answer…do five or six of them, something like that, in the time available. I don't remember. The only one I…the only question I remember is the one that I wanted to answer but didn't have the time, and that was a question I [could tell] Walter Kauzmann had put [in] there. "What would the world be like if the Exclusion Principle—Pauli's Exclusion Principle—did not exist?" First of all, that required some thought.

BROCK: Yes.

TANENBAUM: Because it means all the orbitals, for example, in the atom will fill up and be thermally distributed…

BROCK: Not [forced]…

TANENBAUM: …not [forced by exclusion limits]. So compressibility will change. And bonding, how will you have…. And you won't have the…you'll lose [most of] the discreetness that you had in the chemical reaction.

BROCK: And so, it was after you completed your general exams that you had the DuPont Fellowship, and I think a Proctor Fellowship, is that right?

TANENBAUM: Yes.

BROCK: Now were those…were those given out by the department chair or what was that process?

TANENBAUM: Well, you know, I'm not sure, but I think it was under the dean's level.

BROCK: Okay.

TANENBAUM: Yes. The DuPont…although the DuPont Fellowship was certainly in chemistry, the Proctor Fellowships were, I think, across the graduate school.

BROCK: Oh, okay.

TANENBAUM: The Proctor—one of the founders of Proctor & Gamble…. Actually, I was given two fellowships, but the Proctor Fellowship was an honorary fellowship, because they would only give me one stipend, and so…. […] And the DuPont stipend was larger than the Proctor stipend.

BROCK: Now did that…did the DuPont Fellowship carry with it sort of a tacit agreement that you would go down to the Central Station or go to an…

TANENBAUM: No.

BROCK: …interview with the recruiter or anything?

TANENBAUM: Not at all.

BROCK: No.

TANENBAUM: Not at all. I did do that. But not at all. It was absolutely no strings attached to it, whatsoever, or, [surprisingly], no effort on the part of DuPont to get to know you, either.

BROCK: So as your research is progressing over these two years, I guess as you're closing in on the end of your research, what were your thoughts about your next move, your next step? Who were you talking to about that sort of thing?

TANENBAUM: Well, I was talking to…my principal faculty contact was Walter Kauzmann. Again, a little serendipity came into play. There was an arrangement there in the chemistry department of organizing tours for the undergraduates to chemical industry, which was in the general area, where you could make a trip there and get back home in the same day. There were graduate students usually asked to go along as sort of the…"chaperone" is not the right word, essentially the leaders. I went on a trip with one group to Bell [Telephone] Laboratories, [Inc.]. I had never heard of Bell Laboratories. I was thinking DuPont, Monsanto [Company], [et cetera].

So, I came up here with that group, and I was just bowled over by <**T: 10 min**> what was going on, what we saw. I went back and spoke to Walter, and [spoke with] him. I said, "They apparently have a chemistry department there." He said, "Yes." I said, "I don't know anything about it. Is it a good place to work?" He said, "The best." I had also applied for a postdoc at University of Illinois. His name will come to me. One of the early workers in NMR as applied to chemistry.

LÉCUYER: Yes, [Herbert S.] Gutowsky?

TANENBAUM: Yes, correct. Right, right. I was offered a fellowship there, but then I really started questioning myself, "What do I really want to do with my life? Do I want to teach?" In which case, I had to take the postdoc. But if I want to do research, Bell Labs was probably the right place to go, and that's what I decided I really…I [had done] some tutoring both…in high school I did some math tutoring for kids who were having problems; I did some chemistry tutoring at Princeton; and I also supervised [an undergraduate laboratory during…] my first year [as a graduate student]. Well, you know, it's okay, but it was not really what really turned me on. So that trip up here put me on a different path.

BROCK: What really bowled you over? What did you see? Just the scale of it or…?

TANENBAUM: Well, the…first of all the scale was large, but just some of the research they were doing. That was just in the beginning of the solid-state explosion. I was interested in single crystals. I saw a lot of people doing work here on single crystals. I just…and but you know it was primarily the recommendation that I got from Walter Kauzmann that said it's a good place to go.

BROCK: Did you have any other questions, Christophe, about the Princeton years?

LÉCUYER: I was thinking about your friends or your, I mean, classmates, if you will, at Princeton, what interactions were there among the students there…?

TANENBAUM: I have to say most of the [chemistry] graduate students there were married and were veterans. The apartment house we lived in was about half way between the group of temporary buildings that had built as barracks for the ROTC [Reserve Officer Training Corps] classes [and the chemistry lab]. They weren't ROTC…B2 or whatever they were during the war, and the chemistry lab. So most of our social interactions were with [the students and their families] who lived there in the barracks, as they were called.

LÉCUYER: Thank you.

TANENBAUM: And they were pretty active [socially], too. We had good times as graduate students. We'd get together for bridge, or we'd have parties, or what have you during the evenings. But you didn't have a lot of time for that. But I would say we would…there was something going on at least once a week. Everybody went in very different directions after that. I learned after I had…that there had been a very limited, but steady flow, of people from Princeton to Bell Labs. One guy I had in the class, I didn't really know him well, but he had come to Bell Labs. He had worked on dipole, measuring dipole moments with [Charles P.]

Smythe at Princeton. Then, probably, the later important influence on me here in Bell Labs was Bill [William O.] Baker. Bill was…I think got his degree in '35, Ph.D. in 1935. He was here at Bell Labs. He was leading one of the research groups here, [and] shortly started moving up the [research management] ladder very fast.

LÉCUYER: Was that <**T: 15 min**> [part of] a Princeton [connection]?

TANENBAUM: Yes, that's right.

LÉCUYER: Okay. Thank you.

TANENBAUM: Yes. He was one of the people that interviewed me when I was really looking for a job.

BROCK: I was wondering about that same question and maybe a slightly different form is how you made, once you set your sights on joining Bell Labs, how you accomplished that?

TANENBAUM: Asked for an interview, and got an offer.

BROCK: Who did you interview with, do you recall?

TANENBAUM: Oh, gosh, no, not really. You moved around and saw maybe half a dozen people in a period of the better part of the day. I think they relied very heavily on recommendations from Princeton.

BROCK: Right. Were these people in the chemistry division that you were talking to? Was that…?

TANENBAUM: Chemistry and chemical physics.

BROCK: Were these…can you give us a sense of…well, maybe before I ask that question…so they came back. Could you describe the job offer that they came back to you with after these…?

TANENBAUM: Yes. They gave me what I think was a pretty good offer. I think it was five hundred a month. But I had learned that the fellow who preceded me [from Princeton] got five-fifty. So I talked to them about that, and they agreed that that's okay, five-fifty was okay. But the most important thing [to] my mind is when I got here, first [arrived for work], I was told…asked, "Do you have any strong ideas about what you would like to do?" I said, "No, not really. The only strong idea I had is that I don't want to continue doing what I *was* doing. I want to do something different."

They said, "Well, why don't you look around? Look around and go and talk to people. I will give you a few names to start with, if you'd like, and then, if there are other people in the program that you think [are] interesting…. If you get really interested in one of those groups and if they're interested in you, that's where you're going to start off." But then I was told, "If you have any ideas of your own, any particular ideas of your own that you think are worth pursuing, come and talk to us. If we think they're sufficiently connected to the future of the business, we'll support you doing that." So, I thought that was wonderful…

BROCK: So what did you do? I mean that seems like a very broad mandate to wander around and talk.

TANENBAUM: Well, I did that for I don't know how long, maybe a couple of weeks. What interested me most was not something that was [already] going on, but one of the group leaders—who they called "sub-department heads" then; they're called "department heads" today—Joe [Joseph A.] Burton [said to me], "[…] Semiconductors are the hot thing here. It's known or suspected that there are a lot of other semiconductors other than germanium," which was what they were really…one thing people were really working on at that time. "How would you like to synthesize some of the other materials that we think could be semiconductors, [or which the] literature tells you were semiconductors, cadmium sulfide, for example, things like that? Some of the other elements that haven't been looked at? Tellurium, for example, which is in the right column of the Periodic Table. And grow crystals and measure them, find out what their properties are, and see what we can learn about them." That sounded like a great idea to me.

So, that's what I started doing. I started scouring the catalogs for the purest elements that I could find. The Johnson Matthey's [company], is that the right name? I don't know whether they're still in business or not. It [was a] company that did sell chemical reagents and [metals]. And other places, [like] National Lead [Company] for lead, who was a big zinc producer, then, out of Missouri…but I've forgotten.

BROCK: I don't know.

TANENBAUM: Yes <**T: 20 min**>. And I started collecting a library, and started growing crystals. Shortly after I got started, we determined that [tellurium] was a metal not a semiconductor, at least not [down to liquid nitrogen] temperature. But there came a rumor that over at the Siemens [AG] Laboratory in Germany, a physicist named [Hans] Wolker had found that if you took the elements in the [periodic table columns] just before […] silicon [and] germanium and just after the so-called Group III and Group V, they would form inter-metallic compounds on a one-to-one [atomic ratio] with tetrahedral structure, which is the same crystal structure as germanium, and [that they] are semiconducting.

In fact, in one, indium antimonide, he was measuring very high electron velocities, which is very important to the frequency response in semiconductors in the ten thousand centimeters per second or so range. So we quickly decided to have a look at that, see if we can verify it at all, and also start looking at some of the other [III to V] compounds.

[I found that we could grow single crystals by the Czochralski or "pulling" method, the same method that was used for germanium. First, we melted together equal atomic ratios of the desired elements and cooled it slowly to form an ingot. In that way, we got pretty big grains in the ingot. We then cut out a single crystal grain to use as a "seed." We mounted the seed on the end of a steel rod and we melted the remaining parts of the ingot in a silica crucible. The temperature of the melt was held slightly above the melting point. Using a mechanical apparatus we lowered the seed on the steel rod until it made contact with the melt. We adjusted the temperature of the melt until it just began to solidify on the seed. We then slowly raised the seed. The liquid melt continued to solidify on the seed and we could "pull" out a large single crystal.] We could grow really beautiful crystals that way, and highly pure. I was able to demonstrate that now you could get electron mobilities in the 10^5 centimeters per second range at low temperatures in liquid nitrogen […]. So we did a rather thorough study, and we got out a few publications my first year, which was a nice thing to do.[3] Then another new PhD, [H. (Hank) J. Hrostowski, joined the group]. I was looking at the antimonides, and he started looking at the arsenides, and then [made] gallium [and other] arsenides […. Together] we did the original work on the III to V compounds.

Then, the big turning point for me came about, I guess, a year later, when Joe Burton told me that Will [William B.] Shockley had felt that it was time to really [give] silicon a hard look. It's known that silicon was a semiconductor. In fact, a lot of rectifying diodes were made from that silicon. There had been a few attempts to…like, you could grow silicon, single crystals by the Czochralski Method. The problem […] is [that] the melting point is much higher, [1420]° Centigrade, something like that. The material is [chemically] very reactive, which turned out to be a benefit in the long run [but was a nuisance in crystal growing]. People

[3] G.L. Pearson and M. Tanenbaum, "The Magnetoresistance Effect in InSb," *Physical Review* 90(1) (1953): 153; M. Tanenbaum and H. B. Briggs, "Optical Properties of InSb," *Physical Review* 91 (1953): 1561-2; H.B. Briggs, R.F. Cummings, H.J. Hrostowski and M. Tanenbaum, "Optical Properties of Some Group III-Group V Compounds," *American Physical Society Bulletin* 28 (1953): 9; M. Tanenbaum, G.L. Pearson and W.L. Feldman, "Magnetoresistance in InSb and GaSb Single Crystals," *American Physical Society Bulletin* 28 (1953): 8; and M. Tanenbaum, "Hall Effect and Conductivity of InSb Single Crystals," *Physical Review* 91 (1953): 1009-10.

had tried to grow…had succeeded in growing NPN and PNP structures [in silicon and they] could [make] good rectifiers, but no one had ever been able to make a transistor that could show power [gain]. Shockley decided, "Let's really get serious." So, I was…they asked me if I'd be interested to—I'd shown I knew how to grow crystals—if I'd come over to Shockley's group, the physical chemist, try with the group…try to make silicon transistors. Then there were, really, three of us involved, two members of technical staff, I was the physical chemist. The other was [Leo Valdes], who was the electrical engineer and [would] characterize whatever […] we made. The third one was a senior technical associate, Ernie [Ernest] Buehler, who was a crystal grower par excellence.

LÉCUYER: Okay <**T: 25 min**>.

TANENBAUM: [The structure of the classical junction transistor consists of three regions. In an NPN transistor, the two end regions are doped with an impurity that produces an excess of electrons and are called N-type. The central region, called the base layer, which is very thin, is doped with an impurity that produces a deficit of electrons. In such regions, it appears that the electrical current is by positive carriers (called "holes") and is called P-type. (In a PNP transistor, the impurity placement is reversed.) The regions where the N- and P-type areas meet are called junctions. If a voltage is applied between the two end regions, ideally no current will flow. However, if a variable current is applied to the base region, then current will flow between the ends and replicate the variable applied current. This occurs because changes in the potential of the base region will cause one of the end regions to leak electrons into the P-type base region (in a PNP, it leaks holes into the N-type base layer) causing current to flow from one of the end regions to the other. However, these injected electrons can combine with the "holes" in the base region and become ineffective. The "lifetime" of the injected carriers (electrons or "holes") depends on the purity and perfection of the crystal structure in the base layer. With the proper voltages applied, the current that flows from end to end will amplify the variable current that flows to the base area. In a good transistor, power gain is achieved. Both the power gain and the frequency responses of the transistor are critically dependent on the purity of the silicon and the thickness of the central layer. In germanium, with an injected carrier lifetime of several microseconds, a central layer width of 25 to 50 microns will produce good transistors.

We tried making silicon transistors by adding small amounts of N-type, then P-type, and then more N-type impurities to the melt sequentially while we were growing a silicon single crystal by the pulling method. We were able to produce NPN structures and PNP structures with central regions as thin as 25 microns. Apparently, the lifetimes of injected carriers of even our best crystals was less than a microsecond, and the efficiency of the injected carriers was low. While the structures showed transistor-like characteristics, we could not get power gain. Even with 20 micron widths no power gain was observed. We could not grow thinner layers reproducibly by the doping method. Ernie Buehler then suggested that we could grow thinner base layers by changing the rate at which the silica crystal was grown. This was possible because the rate at which an impurity is incorporated into the growing crystal depends on how fast the crystal is growing. Different impurities act differently. If both the N-type and P-type impurities were added to the melt at the same time and in the proper ratio, NPN structures with base layers less than 15 microns wide were readily produced by altering the growth rate.

However, when I tried to make transistors from the NPN structure by making electroplated metal contact to the ends of the structure and electrically fusing very thin aluminum (a P-type element) wire to the P-type base layer, the base layer turned to N-type and the transistor structure disappeared.

A somewhat similar phenomenon had been observed in germanium. In that case, if N-type germanium was heated and then cooled quickly it became P-type. It was later determined that the cause was a very rapid diffusion of traces of copper, which was a P-type impurity. In our silicon case, the P-type base became N-type. It had also been observed in the germanium that if the crystal was cooled slowly it became N-type again. The copper had precipitated out of solution inside the crystal and was no longer electrically active. Similarly, I found that if I annealed our silicon transistors after bonding the aluminum wire, the P-type layer reappeared and we had the world's first silicon transistor with useful, good power gain.]

But the one thing that that told us was that, first of all, rate growing [was] very difficult to control <**T: 30 min**>, because you're dealing with very, very sharp and relatively small transients at very high temperatures. [In addition], the process of having to anneal afterwards was…

[END OF AUDIO, FILE 1.3]

TANENBAUM: …that process was not going to make a very pretty production kind of process. [During that period], we had a fellow over in the chemical/physics group, Cal [Calvin S.] Fuller, had been studying the diffusion of impurities in silicon. Previous work had been done by a colleague of his, and I can't think of his name…on germanium. He was working on silicon. [He discovered that] donor [impurities] like antimony diffuse [slower] than acceptor [impurities] like aluminum […] or gallium. He demonstrated that you could…if you started with a…let me get this, make sure I get this straight, an N-type piece of silicon and…is that right? No, you start with…it's amazing what happens as the decades go by. Well, at any rate, let me simplify it by simply saying, you could co-diffuse [simultaneously] both an acceptor and a donor, and end up with an NPN structure […]. He was also [using] silicon that had been…was grown by the floating zone technique, if you're familiar…are you familiar with that?

BROCK: Yes.

TANENBAUM: Yes. It turned out that the silicon grown by floating zone did not show [the N- to] P-type behavior of heating up and quenching.

BROCK: Okay.

TANENBAUM: We didn't know why then, but we did find out shortly thereafter. So [Cal] had prepared samples of NPN structures in this material [with P base layers less than 4 microns wide, and then you could heat it up and cool it down and it didn't change, the junction basically didn't disappear. The challenge was we now have these [base] layers that [were a few microns wide but] they're buried in a piece of silicon. How do you make electrical contact? The top layer is very heavily doped N-type. The diffused layer maybe 10^{16} [to] 10^{17} acceptors [per cubic centimeter] were fairly lightly doped, and the bottom is even more lightly doped N-type.

[An] obvious thing to do is […] to melt your aluminum [contact wire through] the top layer, except I felt pretty sure that I would…the dopings were going to be so high that I'd get a degenerate junction [(i.e., metal-like)] and would not…and it would be shorted essentially to…the aluminum wire would be shorted to the top [heavily doped N-type layer]. So, I started all kinds of other techniques of trying to slowly etch through that top layer without etching through the P-layer. The etch rates are different for the two, but I couldn't control that. [I tried] grinding at a bevel angle on the block, which would expose [more of] the [base] layer, but I could never get transistor action after bonding to it. I think it was primarily because I had too much surface exposed and […the injected minority] carriers are reapplied in that situation.

Until, finally, one night, I was at home and my wife was having a bridge party with [lady friends from the neighborhood]. And Charlotte always wanted me to live at least a twenty-minute drive away from the lab, so I would not be too tempted [to go back every night after dinner]. But I agreed and she agreed this was a good night for me to get out of the house. So I came back to the lab and said [to myself], "Well, let's try […] the straight bond through [the top layer] method." And it worked. I could have kicked myself around the lab; I'd been working for a year [or more on complex solutions and the simplest approach was the successful one]. But we made the first diffused silicon transistor that way and with a [very] thin [base] layer, it had frequency <**T: 05 min**> response in excess of 100 megahertz, which was…they couldn't hardly [achieve] that in germanium, except through a diffusion process, [which] was much more difficult [because you had] to keep the copper out. That's what really jump-started the silicon revolution. Jack [A.] Morton who was the vice president of development, I think, was abroad [when] he was informed that we now had a good silicon transistor. He [cancelled his trip, returned to Bell Labs], and […] turned the development department around to working on diffusion in silicon, [and said], "Forget germanium."

It just turned out that there were so many…the only disadvantage of silicon was that the [carrier (electrons and "holes")] mobility is naturally lower in silicon than it is in germanium. So, to get a given frequency response, you had to have better control […] over the base layer. The diffusion process permitted you to do that at will, essentially. One of the problems, of course, is that silicon is so reactive that you worry about surface defects. But as it turns out, you can use that oxide [layer that formed spontaneously on the silicon surface] to protect [the devices] from any atmospheric [contamination], also to serve as an etching mask and all of those kinds of things. [The latter process] was what made the integrated circuit possible.

[Shortly after the silicon transistor work, I was offered a job to lead a new department to investigate the electronic properties of the other promising inorganic materials. We studied gallium phosphide and other interesting inorganic materials such as ionic salts. All of the continuing development work on silicon transistors moved to the development area except for Carl Frosch's continuing work on silicon oxide growth that moved to my new department. It was during this time that the early work on photolithography was also going on. Bell Labs had all the elements needed to invent integrated circuitry—diffusion, oxide masking, and photolithography—but didn't do it. Intel and Texas Instruments did that. That has been a continuing mystery to me].

BROCK: Well, this is such an amazingly fertile period of time and such important developments that quickly discovered that Christophe and I have very many questions about this time period…

TANENBAUM: I was going to ask you…

BROCK: Shall we…

TANENBAUM: Yes.

BROCK: Here, I'll pause it [recorder off].

I guess a question I'd like to start with is when you came in and you had these discussions with…oh, first of all, if you could tell us a little bit about Joe Burton, what his background was, the sort of work he was doing…?

TANENBAUM: Sure.

BROCK: So that'd be great.

TANENBAUM: Joe was a PhD from Johns Hopkins, as a matter of fact. I'm pretty sure that's right, if I'm remembering that correctly. What work was he doing? Well, he was, as I said, a sub-department head. So a good part of his time was taken up just essentially keeping up with what was going on his department. Frankly, I don't really recall what he was…what specific work he was doing, if he was still carrying out laboratory work or not. The people doing the diffusion, both Cal Fuller and the other gentleman who was doing diffusion in germanium, they were part of Joe's department. He was a sub-department head; […] chemical physics […] was a called department ([…] today I think they call it a division).

BROCK: Okay. And he was a chemist.

TANENBAUM: Yes.

BROCK: Okay. So when he proposed the work that we discussed on working on the…following up on the Wolker leads and the III to V Groups, did you join, then, his group?

TANENBAUM: […Actually, I joined his group before that. He wanted to look broadly at other promising materials and I was started down that path when we heard about the III to V compounds].

BROCK: All right. I guess at this time, with the semiconductor work, I'm interested in just if you could help me with a picture of the size and the scope of the general semiconductor work at Bell Labs. How big was Burton's group? Shockley's group? I guess there was also a <**T: 10 min**> development group here under Morton.

TANENBAUM: There were people working on semiconductors wherever you turned.

BROCK: Okay.

TANENBAUM: Yes. It was the principal activity [in the research division]. It ranged all the way from very fundamental theoretical solid-state physics, in terms of what makes a semiconductor and what is the band structure, the detailed energy band structure, in germanium and silicon, and the III to V compounds once they became available. So it was…that was the place to be at that time. How many people were in Joe's department? I would…in general, those departments were ten to fifteen […] members of technical staff.

Then each…just about [every experimentalist] who was productive would have a technical assistant or two. Shockley's group…I think it was a similar size. Then there was the Theoretical Physics Group that was […] about ten [scientists] or so, I think exclusively working on solid-state physics. There were substantial groups over in the development department, much larger groups, who were developing processes and techniques for making germanium transistors and then later silicon transistors and silicon diodes. Then there were substantial numbers of people in what was kind of the systems research area, figuring out, "Now we've got these wonderful little things, what can we do with them?" They were working on everything from television bandwidth compression [to voice recognition and many other aspects of communication systems]. But now that you've got solid-state transistors you don't have to worry about rooms full of vacuum tubes with the tremendous power that they take. So there were all kinds of experimental work going on there, too. If there was anything you could think of, at least anything I could think of, that I wanted some information on—solid-state—I could find somebody who was an expert.

BROCK: So it could be over a hundred people easily…

TANENBAUM: I wouldn't be a bit surprised if there weren't over a hundred people. I'm talking about the full members of the technical staff.

BROCK: Right.

TANENBAUM: [Not including] the supporting staff [or the administrative people who were involved with] the research department. […] In the development department, you'd find a few times that, I suspect.

BROCK: Wow. And the development department was also on this campus, just…

TANENBAUM: Yes.

BROCK: …separated in a different building…?

TANENBAUM: [A large part of it was in another section of the Murray Hill, New Jersey, complex. Another substantial part was in a branch lab co-located with the Western Electric manufacturing plant in Allentown, Pennsylvania. The branch lab concept started] either just before or shortly after I came [to Bell Labs…]. There would be a branch of Bell Laboratories located at the Western Electric factories, [where they] were manufacturing the [devices] that Bell Labs was designing. I think the first one of those was at […] the semiconductor plant there in Allentown. That would have been a rather sizable group as well.

LÉCUYER: So were there any seminars that supported [the research]?

TANENBAUM: Oh, there were always seminars. You could…on any given day you could find at least one seminar.

LÉCUYER: I see, okay.

TANENBAUM: Either an internal one or […] someone visiting, coming from the outside.

BROCK: And was there someone with an overall responsibility for coordinating this broad semiconductor work, and, if so, who was that person?

TANENBAUM: Well, you know there were a [number of management levels]. The vice president of research…I'm trying to think who that was at the time that I came here. I'd recognize the name if I heard it. But I never met him or anything.

BROCK: Okay.

TANENBAUM: Yes. But he would…all the research departments reported to him. There were how many levels of supervision? One, two, three, I guess, four levels of supervision. The fourth level being the vice president, first level being sub-department head, and then <**T: 15 min**> there was a department head, and then there was an executive director, who would have…

LÉCUYER: Right.

TANENBAUM: …a few departments there. I think there were probably, maybe, only two executive directors of research at the time that I was there. Bill Baker was Vice President of Research for a long period of time, before he became president of Bell Labs. Prior to Bill was [an Executive Director of Research]. I can see him. [His name] may pop-up.

[The structure was similar in the development area. All of device development reported to Jack Morton at the vice presidential-level. Since the vice president of research reported directly to the president, it all came together at that level. However, there was much interaction between the managers of research and those of development, as well as between the members of technical staff (MTS) at the bench level.]

BROCK: I guess it was in 1952, the year that you joined, when Bell Labs played host to that first big symposium on the transistor technology for its licensees. Were you here in time for that? Did that…?

TANENBAUM: Not for the first one.

BROCK: Okay.

TANENBAUM: Not for the first one, which was all germanium. Yes.

BROCK: Right.

TANENBAUM: But I was an active part of the second one [on silicon], but I don't recall [exactly] when that occurred, but it would have been the middle 1950s, 1955 or [1956].

BROCK: Well, let's see. Where to go from here? I think that, unless you have a different idea Christophe, that by February 1953, which seemed very quick to me, you published an article in *Physical Review* with Gerald [L.] Pearson on these…[4]

TANENBAUM: III to V compounds…

BROCK: Right.

TANENBAUM: Indium antimonide.

BROCK: Indium antimonide…yes.

TANENBAUM: Magnetoresistance, if I recall…

BROCK: Exactly right. I was wondering if you could talk a little bit about Pearson's role in that work with you, and him, and your collaboration, and…

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[4] Pearson Tanenbaum, "The Magnetoresistance Effect in InSb."

TANENBAUM: Yes. Gerald was over in Shockley's group, I'm pretty sure. He was certainly over in the physics department. I gave […] a paper seminar on indium antimonide, and Gerald, I think it was the next day or something, either came by [my lab] or called me up. I had mentioned Hall effect and he said, "Well, maybe we ought to look at magnetoresistance." I said, "What's that?" I was a physical chemist; I didn't know very much about those things. [I had learned about] the Hall effect [but not magnetoresistance].

So he told me that. I made some crystals for him. He did the actual electrical measurement of the magnetoresistance. I had done Hall effect measurements, but he did magnetoresistance. That sort of thing would happen all the time around Bell Labs back then, where there's just individuals hearing about something, we'd walk out in the hall and talk to you about it, and maybe figure out something you wanted to do together.

BROCK: And it was…and obviously, the structure was open to having people do that.

TANENBAUM: A lot of the openness was really expected. People would…

BROCK: I think in that work with Pearson of early '53, your sample…it was purified using the zone refining technique. So I thought maybe we could talk a little bit about…this seems like a fairly early adoption of zone refining and that it's…as I understand it was another local development…

TANENBAUM: Sure.

BROCK: …so could you tell us a little bit about the place in zone refining as you found it?

TANENBAUM: Sure. Well, zone refining was invented by Bill [William G.] Pfann. Bill…it was…he invented it, essentially, to purify germanium, and it worked out just extraordinarily well for germanium. I don't know where I first heard about zone refining, but it was one of the techniques that I wanted to use to purify the metals, the elements that I was getting to make compound. So I went down to talk to Bill about zone refining, <**T: 20 min**> and how does it work, and how can you best do it?

He said, "Well, for something with germanium, a single pass is usually all you need to really clean up the germanium." But some of the other metals, I guess, I probably said, "You know, I'm going to want to do many passes," and wanted to find out, can I use separate heaters to do it all that way. He said, "Yes, you can do that." But, he said, "There's another technique that I thought about that [I just] never really [tried]. And that's to build a reciprocating zone refiner," which would move [one or more zones simultaneously. You worked with two or more heating coils in series. That assembly passes along the ingot], and then […] arrange through a transmission mechanism [and] a switch for it to reverse quickly and then start another [series of passes through the ingot]. I said, "That sounds like a good idea." So I went to the storeroom [for parts] and got some stuff, and bought some motors and a transmission gear that would permit for the quick reverse, and built a reciprocating zoner.

Again, just one of the great things about this place was that you'd get advice, could find an expert on just about anything you wanted to do.

BROCK: Was this…I guess what I'm curious about, was this work at a very unusual level of chemical purity that you were dealing with in this work, you know, compared to work out there, elsewhere in the chemical community? Did it…?

TANENBAUM: Sure.

BROCK: Was it unique in that way, or distinctive in that way?

TANENBAUM: Well, you were dealing with solutions of impurities that were really micromolar, […one part per million or less]. If you wanted really pure stuff, you'd go down to [one part per billion. I, at least, was not aware at that time of any place where you needed those kinds of purity or knew how to measure them.

We could measure them electrically, of course, [by their effect on semiconductor conductivity]. There [was also] a lot of work on mass spectroscopy, to not only measure, [but] be able to identify [the impurities, essentially. Because you could probe…Machines were built here so that you could probe with an arc point, and with a mass spectrograph, just pick up the fumes you produced…the vapors you produced. You volatilized them…

BROCK: Uh-huh.

TANENBAUM: …to see what the uniformity of the concentration was, what the elements were. There was a lot of work done […] in microchemistry.

BROCK: Right. Did that have any implication for the facility? I mean dealing with such high levels of purity, I guess, and such great concern about controlling the contaminants, did the facility have to evolve in any way? I'm thinking of cleaning it up…

TANENBAUM: Yes. No, not at that stage of the game. That became very important later on, particularly with integrated circuitry, where…the real problem for particulate problems: laminar flow. You've seen a laminar flow room. That was an invention out at Sandia Laboratory, [a subsidiary of Western Electric at the time that later became Sandia National Laboratory], that was imported into the semiconductor industry. But you got the…for the most part you were dealing with solids, so once the solid was made, the only thing you had to worry about was surface impurity.

When I first came here, Bell Labs was not air-conditioned. There were a few air-conditioned rooms, which were very popular [in the summer], but you had to make a special case for the requirement for a temperature- and humidity-controlled room. There was one right across from my laboratory. There were certain etching facilities in there that I found very essential when the temperature was high. [laughter]

BROCK: Let's see. Did you have any question right here, Christophe? Okay. Well, just press on. I think it was in the same year that you joined, was when <**T: 25 min**>, [Gordon K.] Teal and Buehler had developed a technique here for pulling these single silicon crystals using a silica crucible. Was that…that was Teal…did he depart before you had a chance to meet him? Or what was the…

TANENBAUM: I…

BROCK: …impression of the work?

TANENBAUM: Yes. Well, I met him very, very casually. It was all ready known that he was…when I met him, it was known that was leaving and going to Texas Instruments. Yes. But he was…I think he was still on the premises, but I think the overlap had to be a matter of weeks or so.

BROCK: Did…?

TANENBAUM: Oh, and by the way, we did skip over it, as you may well know: the impurity that was causing this conversion to P-type silicon when you heated it up and quenched it, was oxygen. There was some really beautiful work done. It was done, I think within a year after I had had my experience and had to use annealing techniques to get […] the transistors. Wolfgang Kaiser, who was…I'm pretty sure he did his doctorate degree in Germany, but was on the [Bell Labs] staff […]. He left a year or two afterwards [to return to Germany. While at Bell

Labs] he demonstrated, in the most convincing manner, that it was oxygen. When you heated the silicon up and then cooled it down slowly, so that you didn't get the P-type thing, then, if you [looked with infrared] through […] the silicon, […] you could actually see the oxygen clusters.

BROCK: [Okay].

TANENBAUM: Then you could heat it and you could see them dissolve and [the sample] would turn into N-type.

[One of the things that we had observed in crystal growing was that if we didn't rotate the crystal to stir the melt and pulled the crystal up slowly, the N-type effect was much weaker. We didn't know why, but, clearly, what was happening was that the oxygen from the silica crucible that contained the melt was dissolving into the molten silicon. Stopping the stirring slowed the oxygen dissolution rate and its incorporation into the growing crystal. As the newly grown crystal cooled slowly, the oxygen atoms precipitated in clusters in the crystal and did not affect its conductivity. However, if you then heated the crystal and cooled it rapidly, as I did in bonding the aluminum wire to the base region in my early rate-grown NPN structures, the oxygen clusters would dissolve and, acting as an N-type impurity, caused the thin P-type region of the NPN to disappear].

BROCK: Well, it seems like '52, that was also the year in which Fuller really develops his diffusion technique for doping. Could you tell us a little bit about…?

TANENBAUM: Fifty-two or '53, but it was certainly in that area, yes.

BROCK: One, tell us a little bit about your impressions of Calvin Fuller, but also, what sort of splash, if any, his diffusion work made here at Bell Labs?

TANENBAUM: The diffusion work by itself, not great. I mean it was a pretty straightforward kind of thing to do. There was interest in how rapidly impurities moved in germanium and in silicon, because the processing techniques you do to heat the substance up, you want the impurities to stay [put]. Also with diffusion, you can get diffusion gradients, you can get concentration gradients, and that gives you built in electric fields. And there were some transistor designs where that looked kind of interesting, because you could get a little more, little higher velocity in the [injected holes or] electrons [across] the base layer.

But it was, you know, a straightforward…just a question: impurities must diffuse, we ought to find out how fast they diffuse and how we can control them and so forth?

BROCK: And was Fuller…he wasn't working in your group, was he?

TANENBAUM: No. He was in Joe Burton's group.

BROCK: […] Burton's group.

TANENBAUM: Yes. Yes, I'm pretty sure Cal was in that group. But he was not in Shockley group.

BROCK: So was…so to go to the shift, really, was in '53 for you, when you moved over to Shockley's group, is that…?

TANENBAUM: I think so. I think so <**T: 30 min**>. It was about a year after I got there, I think. By the way, there is another name I should mention, another chemist. When this group, which was sort of an informal group, was set up to look at silicon, [N.] Bruce Hannay, N.B. Hannay, N…I don't know what the N stands for […].

[END OF AUDIO, FILE 1.4]

BROCK: Great.

TANENBAUM: And, you know, Bruce would stop by on occasion to see how things were going, but he really did serve as a very good interface with Cal Fuller. I dealt with Cal directly, but he was very familiar with what Cal was doing, and was an advocate of looking at the diffusion techniques, as well as crystal growing techniques.

BROCK: And he was a chemist as well.

TANENBAUM: Right, Princeton chemist as well…by [coincidence].

BROCK: Did you…in making that shift in '53 to this informal silicon group, did you have conversations and meetings with Shockley about the goals for this silicon push?

TANENBAUM: Sure. Yes.

BROCK: Could you tell us about that?

TANENBAUM: Well, at first I had a lot of conversations with other people, because I was told Shockley was not an easy guy to work for. But as it turned out, you know I was the only chemist in the group there. So I knew things Bill Shockley didn't know, and he knew that. He knew that. He and I got along just fine. Shockley was very, very tough with [the] physicists who worked for him. He was extraordinarily bright, intelligent guy himself. He set very high standards. And if people didn't always meet them, then he was pretty rough. He was pretty rough with them.

But I really didn't have any problem at all working with Bill. Bill was, I think, also [at] a point in his life where he was starting to think about 'was there something else he wanted to do.' So, he wasn't around an awful lot. He would go to a university or someplace, where he'd give some seminars. He would stay for a few…a week or two, and traveled abroad about…a lot of the time. So, I really didn't…I didn't really see a great deal of Bill. But as I say, when I did, he was interested and supportive, helpful.

BROCK: How did he frame the proposition of getting into the silicon work? I mean did he express a vision of it that, you know, "Silicon's going to be the way to go in the semiconductor work," generally?

TANENBAUM: No. I don't think it was clear that silicon was going to be the way to go, because people tried to go that way and then there were problems and things about silicon that we didn't understand. Even growing silicon, good silicon crystals, wasn't all that easy, too. There weren't many sources of pure silicon. DuPont was the principal source at that time. It took someone like Ernie Buehler to figure out what you needed to grow them.

He would…I would watch him. You'd get him a batch from DuPont and it was mixed crystals, loose, [small] crystals of all [shapes]. He'd spread them out on a piece of paper. Then he'd look at them and he'd pick [them out] and say, "That's a good one. That's a good one. That doesn't look too good." He was just looking at the appearance of the crystal. Is it really shiny […]? Well formed? It was an art, but an art that he developed very well. He grew…tell him what kind of silicon crystal you wanted, and he'd grow it for you.

BROCK: So there were…the three of you really, Buehler, Thomas, yourself, with some oversight from Hannay in a coordinating role. Did the three of you move to a common laboratory to pursue this work? Or how was that…?

TANENBAUM: Yes. Well, Ernie's laboratory was…I could show you these places, if want us to walk around. I'm sure they look different now. Ernie's laboratory was maybe a hundred feet from mine. You couldn't move [his] very well, because it was full of great big RF [furnaces…]. Don Thomas's lab was right next door to me. But I really didn't have too much interaction with Don, because I didn't have anything for him to measure for a while <**T: 05 min**>.

At the time, right next door to me, on the other side, was Charlie [Charles A.] Lee, who was […] trying to make a transistor by diffusion in germanium and developed a really very, very clever technique. What he did was get a big germanium crystal, cut a recess in it, and make a [germanium] lid for it. […] Then [he] put a sample in there, along with the impurity that [he wanted] to diffuse, [put the lid on], put [the assembly] in the vacuum, […heated] it up, [and the impurity diffused into the sample…]. The large crystal [container] acted as a sink for any trace copper that was around. He could get around [the copper problem] that way.

Just a few months before I made our silicon transistors, he made a successful germanium transistor with a very, very narrow base. I think it had a frequency response in the neighborhood of 400 megahertz or so. You know it really, really looked terrific, but, again, how do you mass-produce something like that? Once we had silicon, germanium quickly [disappeared], because the properties [of silicon] were so much better. With diffusion, it looked like we could reach whatever frequency that we really wanted [with silicon].

BROCK: Were there…so this is '54 through early '55, which is I guess when the real push…your development was going on. Were there other groups at Bell Labs who had a similar goal of making a silicon transistor by whatever doping route or…?

TANENBAUM: Yes. There was a group in the development department, nobody else in the research department.

BROCK: Okay.

TANENBAUM: But there was a group in the development department that [was] looking for techniques [for] doing that. Alloying was one of the…alloying was the technique that was being used very successfully in germanium transistors, where you take a piece of N-type germanium and then alloy in from two sides with a deposit of a aluminum. Essentially, by…and then by carefully controlling temperature [to create PNP structures with a base layer 30 to 50 microns thick].

There was an effort to try to do that with silicon. The real problem…the surface [oxide] became a real problem there. Aluminum, if you heated it well above the melting point, would [chemically reduce] the oxide [layer] and alloy [with the silicon]. But you couldn't get clean silicon without the oxide. So while you are depending on the fact that you need a […] thermal pulse to get the aluminum through [the oxide layer], then you've got to stop [the alloying action with the pure silicon] before it goes all the way through [the sample]. You [needed base layer widths of no more than 10 microns. The alloying technique] was never successful.

BROCK: And was that a similarly-sized group trying that over in development?

TANENBAUM: I think it was…I think it was larger.

BROCK: Yes.

TANENBAUM: But you know it wasn't immense. The fellow who was involved in that was [Nick] Holonyak, [Jr., …]. He worked with me a little bit trying to figure out better techniques for getting clean silicon and making the alloy work was very, very difficult. You deposited a layer, a thin layer of…. [We tried evaporating] a [thin] layer of aluminum on [a piece of silicon]. The piece of silicon was as clean as you knew how to get it. As soon as it…just as soon as you reach the melting point of aluminum, [surface tension caused] the film [to] break up, and […] become a bunch of little dots […]. Just a messy process.

BROCK: So it seems…so was it in, I guess, it must have been then in '54 that you achieved success with your rate-growing transistor. Then it was early in 1955 that you had the diffused base.

TANENBAUM: That's right. That's right, [in January 1954 and March 1955, respectively].

Yes. I should have brought that with me. I have the <**T: 10 min**> copies of my notebooks from that period.

BROCK: What about something maybe to talk about before the tape goes off…?

TANENBAUM: Sure.

BROCK: That would be…so what was the reaction to getting a power gain with the rate-grown transistors in silicon? What was the…did you announce that internally…?

TANENBAUM: Oh, sure …

BROCK: I mean…

TANENBAUM: Sure.

BROCK: …what was that story like?

TANENBAUM: Sure, sure. No. I called my boss. [laughter] People came around to see it. But I wasn't very enthusiastic about it. I knew there was…others were not either. I don't think they ever really, seriously considered it in the development department. It just looked like it was going to be too much of a problem.

I gave a paper on the rate-grown transistor at an off-the-record meeting that the IEEE [Institute of Electrical and Electronic Engineers]—I guess, it was called the IRE [Institute of Radio Engineers] back then—[in 1954]. [A group of IEEE members] had started a series of off-the-record meetings for people working in semiconductor devices. We'd get together and give papers. [In order to encourage people to be open], the papers were never published, and there was no record of the meetings. I've gone back to the IEEE to find out if anybody has any record. I can't…because I don't even remember the place where the meeting was held. I think it was in Colorado—but I'm not sure of that—where I gave a paper on it.

It was at that meeting, after I gave my paper, that Gordon Teal stood up and said, "We made a grown junction transistor at TI [Texas Instruments], also." I said, "Gee, that's interesting, Gordon. Will you tell us about it?" He said, "Well, I'm sorry, I can't." But later I did learn that they had simply used double doping technique and, apparently, were able to make some silicon transistors in that way. They were selling them, I think, just to the Department of Defense. [But] once the diffused transistor came along, it disappeared.

BROCK: Did you have a question here, Christophe?

LÉCUYER: I was thinking that manufacturing was the key component in the decision-making process.

TANENBAUM: [What good's a transistor if you can't manufacture it?]

LÉCUYER: […] I was also thinking about the difference between the Bell Labs and TI, because TI said that, to do it over…they knew that it was difficult to manufacture it, right. Nonetheless, they actually produced it and sold it.

TANENBAUM: Yes. [I don't think it ever] reached the [broad commercial] market.

LÉCUYER: Oh, I see. Okay.

TANENBAUM: [I understand] it was […] sold in small quantities [at high prices] to the military, who were interested in it, primarily, because of the high temperature [at which it could operate].

LÉCUYER: Okay.

TANENBAUM: Germanium you heated to…you can't heat it as high as boiling water. […] Devices made [of] germanium [were very temperature sensitive]. The early computers made of germanium had to be air-conditioned in order to be stable. So the military were very interested in the [higher] temperature. But how many were made and sold, I have no idea. But I know there was no place you could call up and buy TI silicon transistors.

LÉCUYER: Okay.

BROCK: I'm just thinking, so after achieving…

TANENBAUM: By the way…

BROCK: Oh, yes?

TANENBAUM: I'm sorry. I'd like to say one other thing. You never would have gotten integrated circuits with the grown-junction transistors. Geometry is just awful.

Go ahead.

BROCK: Did you have a follow-up question there, Christophe?

LÉCUYER: No.

BROCK: You sure?

LÉCUYER: Yes.

BROCK: I was just going to say that having…setting your sights, then after achieving success with the rate-grown transistor, you know, looking forward to doing the diffused-phase, right around this time period, I think in '54, I think that's when Shockley was out at Cal Tech [California Institute of Technology] on leave or something at Cal Tech. There was some transition in the management of Bell Labs at that time. Did either of those developments have any effect for you in your work?

TANENBAUM: No. No. I think <**T: 15 min**>, if I remember correctly, Morgan Sparks took…became my boss when Shockley officially…after Shockley officially left (when he was on leave or what have you, he would come back occasionally). [Morgan was another chemist], very active in the first grown-junction germanium transistors, did some of the important work back then.

BROCK: And he basically replaced Shockley, when he…when Shockley left.

TANENBAUM: I think that's right. That's right, [at least as far as I was concerned].

BROCK: Well, could we…did the group of people you were working with change when you really started to work in earnest on the diffused-base silicon transistor or was it Thomas, Buehler, maybe Calvin Fuller a little bit more? What was the constellation of people you were working with on that effort?

TANENBAUM: Well, it was…you've named them.

BROCK: The same group.

TANENBAUM: [It was a somewhat different group. Cal Fuller was providing the material, the diffusion materials. I was working to try to get hold of that base layer, electrically, and Don Thomas had replaced Leo Valdes.]

BROCK: Right, right. And that was the real crucial step for integration, I guess, is what I'm hearing, you know: that you can get materials from this person; Thomas can test it; get crystals from the other person. That's really…

TANENBAUM: Sure.

BROCK: …making that connection, then, is the real critical problem of integrating these techniques and getting a working device. Is that true?

TANENBAUM: It certainly facilitated things tremendously, just no question about it. Would I have moved to diffusion if Cal Fuller hadn't been there? Maybe. I don't know. It's never an issue. I mean, material just showed up: "Why don't you try this?"

BROCK: Was there great…it sounds like there was very little or we haven't talked about any rivalry yet, but a very open, you know, free exchange?

TANENBAUM: I think there was some rivalry between the development department—who, Jack Morton has said, "Make a silicon transistor"—and the research department. You really need to talk to someone over there, because I didn't think about it very much. I was busy doing what I was doing. But there were people there trying to make a silicon transistor, too.

BROCK: Right. Well, having…well, let's talk a little bit about…shall we focus in on the diffused-based transistor a little bit more, Christophe? Could we just go back into, again, the story of the real critical developments, as you see it, for actually achieving the functioning device? What were the critical obstacles and the critical things to overcome then?

TANENBAUM: Well, I was…shortly after we made those first devices, I was offered a promotion, essentially to put together a group to work in other areas of solid-state physics […]. I did very little more about the silicon diffused-base transistor. That moved quickly, immediately, essentially, over to the development department. There was some obvious problems with the structure as I…in my first transistor, although, there was no short between the aluminum wire and the N-type layer. There was the junction there <**T: 20 min**>, in fact the insulating junction between the P- and the N-type, very highly doped, and, therefore, with relatively large capacitance, electrical capacitance.

BROCK: Okay.

TANENBAUM: In fact, that was probably what limited the frequency response on those first transistors. That was just not an ideal job to me. That problem was solved, not at Bell Labs, but out at…I don't know whether it was Shockley Semiconductor then, or whether they had gone to Fairchild. I think they probably had gone, left Shockley, the group left Shockley. I can't think of the guy's name [Jean Hoerni].

But he developed a structure, [the planar transistor], using the oxide layer as a mask for being able, from a doubled-diffused structure, essentially to get connection to the base layer without having the direct contact between the electrical lead and the heavily doped N-type layer. That was an important step forward. I think that kind of lead directly to [Robert N.] Noyce…to Noyce's ideas about integrated circuitry. How the TI people got there, I don't know. I just don't know that story at all.

But we had actually…I had actually tried to make something that you might have called an integrated circuit earlier on. [Robert L. (Bob) Wallace, Jr., from] the systems research area […] came over and said, "I wonder if we can make a structure, make a circuit […] with this technique? Is there any way to build resistors or capacitors in here?" […] What he was […] looking for [was] an RC [resistor-capacitor] circuit [with a connected transistor on a single piece of silicon].

I fooled around with that a bit, not using photolithography, [which had not yet been developed], unfortunately, but using wax, essentially, a black wax that was used [to make strong etching solutions. I] made a wiggly structure like a [resistor] and capacitor-like structure, very crude, connected together with […] gold-compressed wire. Bob […] became ill, and I don't think he ever got back to work, and I got promoted and forgot about it. I don't know whether it worked or not. But it certainly wasn't anything worth having […].[5]

BROCK: Well, this…I did read about Jack Morton's reaction to your success in making this diffused-base transistor. It seems that it was almost akin to a conversion experience for him, and really, you know…

TANENBAUM: Epiphany?

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[5] See U.S. Patent 3,022,472, issued 20 February 1962.

BROCK: Yes. Maybe that's…that's probably a less-charged term. But I mean was it an epiphany for him? I mean […] he came back from Europe and…

TANENBAUM: I think so. I think he saw immediately the great advantages of silicon over germanium. That's why he had a group over [there] trying to make devices that way.

BROCK: And after that, did he have his development group come and get you to transfer what you had done…

TANENBAUM: Oh, sure.

BROCK: to them or…?

TANENBAUM: Sure. Sure. Oh, sure. Yes. It was wide open. We were generally wide open. We had visitors from all over the place. One of the ones I remember most was <**T: 25 min**> […] Gordon Moore. Gordon had just joined the group out at Shockley's lab, physical chemist…well, actually, you know…

BROCK: Yes.

TANENBAUM: …you've interviewed him. Gordon came and spent a few hours. We told him just exactly what we'd done, and how we'd done it. They were…we had a cross-licensing agreement. But, generally, you could find out anything you wanted about what was going on at Bell Labs if you had a cross-licensing agreement. And assuming that our patents had been filed…

BROCK: Right.

TANENBAUM: That was done very quickly too. In fact, I think the general understanding was that if the patent hadn't been filed within six months of disclosure, you could go ahead and publish.

BROCK: Wow.

TANENBAUM: That kept the pressure on the patent department. I'm sure it took six, seven, or eight months, that after long negotiation that you might get another month or two. But then that was very clearly the deal.

BROCK: So it was in, I guess later in the year, in June of '55 at this IRE device conference in Philadelphia that I think you made your public announcement about your achievement with the diffused-based transistor. Could you tell us a little bit of that story, what the reception was? Did you give the paper?

TANENBAUM: Yes, I'm pretty sure I gave a paper. I'm trying to remember that. I haven't thought about that for quite a little bit, for quite a while. I'm just trying to remember whether…how that really went. We published in the *Bell System Technical Journal*.[6] I guess there were two reasons for that. One, they wanted it. Secondly, we could get it in immediately [in] the next edition. So my recollect—and I think there was a press release also at the time of the publication—[…] my recollection is that it was already rather well known that the transistor had been made. I really…I'm having trouble remembering the circumstances. I suspect it was an invited paper. I also gave a paper on diffusion in semiconductors, at that time, diffusion junctions, what the various factors are in terms of how you apply the impurity, and what kind of distributions you'll get. I learned a lot about calculating distributions during that thing. But I'm just having difficulty really remember…

BROCK: I guess what I'm most interested in is hearing your thoughts about what the reaction of semiconductor community, if you can call it that, to that development.

TANENBAUM: Well, the…I think my experience with that reaction primarily was hordes of visitors. Then there was the seminar, too, the meeting that was held on silicon. And it was a very satisfying experience, I guess.

BROCK: So, it was…?

TANENBAUM: There was a great of interest.

BROCK: How did that affect your career, and your standing here…? Your reputation here, and more broadly?

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[6] M. Tanenbaum and D.E. Thomas, "Diffused Emitter and Base Silicon Transistors," *Bell System Technical Journal* 35 (January 1956): 1–22.

TANENBAUM: Well, I […] got promoted, which I thought about. I had to think about it, as to whether I really wanted to move in that direction. Because my own feeling about that was that once you became responsible for the work of a bunch of other highly talented people, the one thing you didn't want to do is compete with them <**T: 30 min**>. I think that was generally the…[some] people, when they got a management responsibility [at Bell Labs], some people would continue a laboratory, and keep on. But that was the exception rather than the rule. I did do one or two…

[END OF AUDIO, FILE 1.5]

BROCK: Okay.

TANENBAUM: One of the things that Shockley was very interested in was the so-called PNPN device. The lab was very interested in that from [as] an on/off switch. When you make telephone connections you use on/off switches [called cross-points], and they were all electromechanical [relays]. You could do it with a transistor circuit, but it took a couple of transistors and a couple of other components. But [with] the PNPN, you could do it [with a single device]. But one thing you needed…because you needed to control the breakdown voltage [at which] a switch would turn on very accurately and you needed very uniformly doped silicon to do that. I became aware of a transmutation reaction, nuclear reaction, where [a naturally occurring] isotope of silicon, bombarded with thermal neutrons, transmuted into phosphorous. It seemed to me like, […] since that isotope was very uniformly distributed, […] that might be a way of getting [the controlled breakdown]. So, we made some samples and sent them out to [the high flux thermal neutron reactor at the AEC (Atomic Energy Commission) lab in Idaho (Idaho National Lab). The samples were] bombarded for several months. That's what was […] required because of the flux [density]. We got [the samples] back, and then we had [wait several months for the residual radioactivity to decay].

I was working with a technician, [Arlene Mills], who was doing [the experimental work]. We got very, very…we could not measure any variation in the [resistivity]. But by then we had realized that really making reproducible PNPN required more than just uniform silicon, so that resulted in a patent [and a publication], but nothing else.[7] I also got interested in a barium titanate, which is a ferroelectric device. It grows from…the way [barium titanate crystals were] generally prepared was growing it from a [molten] salt solution. [The resulting crystals were] what we called "butterfly twins," where a twin would develop and then the crystal would grow [into two plate-like structures joined together which] looked just like a butterfly. […] I had some [knowledge of] crystal growth and how the growth rate will depend

[7] See U.S. Patent 3,076,732, issued 5 February 1963.

upon edge [growth, so…] if [the seed was] a twin, [those edge plates would] grow. So we wrote a paper on that. That may have been just a memo.[8]

So there were a couple of things like that that at the time we handed over, but I pretty much removed myself from laboratory work and research.

BROCK: So in the…well, a couple of questions…but just to follow that line for a moment, the…your promotion was to the sub-department head level? What was your sub-department that you were…?

TANENBAUM: Well, let's see if I can remember who was in it. We hired one [PhD], [K.] Knox, what was his first name? Unusual first name. Inorganic chemist, and he was interested in crystal field theory and experimentation […], so that took us off on an altogether different direction. It related to sodium chloride and color centers, and what have you. Frosch…gosh, what's his first name? Carl, Carl [J.] Frosch, he was the guy who really discovered the [diffusion] masking effect [of silicon dioxide], and how it could be used [in silicon device production].

We hired another physical chemist, [D.] Gerstenberg, something like that, because I wanted to see if we could grow gallium phosphide, a single crystal. [Gallium phosphide] has a very large energy <**T: 05 min**> gap and could have been a very interest…we thought a very interesting electroluminescent material. He tried vapor phase crystal growth, and some things of that sort. Carl Frosch, also tried some gallium arsenide and vapor phase crystal growth. So those were the kinds of things that—I have to go back and look at an old telephone directory if I have to—but those were the kinds of things…. They were chemically oriented from the viewpoint of [an] interest in electronic properties, and…

BROCK: And this was called a chemical physics group.

TANENBAUM: No, no. I don't even remember what the name of my sub-department was. But I was in that job only about, I don't know, a year or two, I guess. And then I was offered a job as an assistant director of the metallurgical department. That's when we came to the super-conducting magnet materials. The name of that department was soon changed to "Materials of Science." But what it was primarily a group of inventive metallurgists who were interested in metals, magnetic metals, metals that were used in vacuum tubes, and things of that sort, electronic use of metals, effectively. But—and I think this was very much [at] Bill Baker's stimulation—they were trying to get some more fundamental work going on in that department.

[8] M. Tanenbaum and A.D. Mills, "Preparation of Uniform Resistivity n-Type Silicon by Nuclear Transmutation," *Journal of the Electrochemical Society* 108(2) (1961): 171-6.

I was asked to come over and to head up a…well, actually, I came over to head up first, a sub-department, and then later became assistant director of the [department]. One of the people in that group was a fellow named Gene [E.] Kunzler, a physical chemist from University of California, Berkeley. [He had] worked with [William F.] Giauque there, [making measurements at] low temperature, ultra-low temperature. He was…they were trying, in the metallurgy department, to [produce] very highly pure copper. [Kunzler] was [measuring] the properties of copper at very low temperature, electrical properties, to determine the band structure of copper and try to understand why it's such a darn good conductor from basic physical principles, working very closely with the theoretical physicists over in the physics department.

Well, one day, into my lab walked Rudy [Rudolf] Kompfner, who was over in the systems area of research. The [solid-state] maser had been invented a year or two before. Rudy was a very, very interesting guy. He was educated as an architect, and got interested in electronics and was the inventor of the [microwave] traveling wave tube. He was not a physical chemist. But he was very interested in masers. They were [evaluating] masers [to determine how they might be used in] telecommunications systems. [Masers are] very low-noise amplifiers. [Rudy wanted to investigate maser properties at very high] frequencies, and the higher frequencies [required] high magnetic fields [for maser operations]. But Rudy needed] to work [at] liquid helium temperatures to keep thermal noise down. You [need large electric currents] in order to get high [magnetic] fields, [but] you couldn't put [ordinary electromagnets] inside [the maser device structure] because it dissipated heat: there goes your liquid helium. So, he came over and said, "We'd really like to try a superconductor, try to make a superconducting magnet [that will operate at liquid helium temperature and not dissipate any heat]." The trouble is that the superconductivity and magnetic fields don't like each other very much. As you…if you put [an ordinary] superconductor—this was discovered by the discoverer of superconductivity [H. Kamerlingh Onnes]—[…] in a magnetic field, [and increase the magnetic field to] a few hundred Gauss […], superconductivity disappears. That's pretty well understood now why that happens.

But <**T: 10 min**> Rudy said, "We understand that there [are some new] materials that [become superconducting at somewhat] higher transition temperatures, and we wonder if it wouldn't be possible to [use them] to make a good, [high field] superconducting magnet." He was looking for a field in the order of ten thousand Gauss, and just fantastic as far as he was concerned. I went in and talked to Kunzler, who was a…he was very interested in applications, as well as the fundamental work that he was doing. He said, "Yes. Let's give it a try." So we talked also with—I'm terrible with names today—Matthias, Bernd [T.] Matthias, who discovered some of the materials for very…with relatively high transition temperatures, 20° Kelvin, or what have you. It did appear that there was some correlation between them, where superconductivity disappeared in a magnetic field and its transition temperature. If you were well below the transition temperature, you'd go to a higher magnetic fields. So you'd like to get higher magnetic field materials. But those materials that he discovered were ceramic-like, and figuring out how to wind a coil with a ceramic was difficult.

BROCK: Yes.

TANENBAUM: So Kunzler started with [some metal alloys…], and Matthias suggested some alloys we might try [like] titanium [plus] vanadium, [which] started looking very promising, I think. I think we probably got up to ten thousand Gauss with that. But there's still interest to see how far we could go. So we…again, Ernie Buehler comes into the picture with some ideas as to how possibly you make a coil with Nb_3Sn, [which] was the material with the highest [known] transition temperature [but a very brittle] ceramic-like material. So between him and Kunzler, they decided, "Well, let's take a tube of metal—silver—and let's fill it with the proper mixture of niobium powder and tin powder." Okay? "Then let's [work] it down into a wire"—and that's where some of our [excellent] metallurgical [technicians] came in—"we'll wind [a coil out of the wire] and then we'll heat it up and see if we can get the tin and the niobium to react and form Nb_3Sn."

During this process, while Kunzler was working with the metals, he [negotiated some incentive]. I [agreed] with him that for every two thousand Gauss he improved, I'd buy him a bottle of scotch. With niobium tin there's something…the titanium/vanadium, I think I probably owed him three or four bottles of scotch. But they made this Nb_3Sn magnet and started cranking out the magnetic field and went past ten thousand, twenty, they got up to hundred-twenty thousand or so Gauss. He let me off the hook with two cases…

BROCK: [laughter]

TANENBAUM: But that was a very, very, very exciting time. Of course, if you go [to a hospital] and get yourself an MRI [Magnetic Resonance Imaging], you're using [one of our] superconducting magnets…[much more highly developed, of course].

BROCK: And did the company commercialize superconducting magnets? Or how did that…?

TANENBAUM: Well…

BROCK: Licensing arrangements, I guess.

TANENBAUM: Yes. Oh, yes, sure. No, there were patents […]. But…and you know there's so many [potential] applications you can think of, from magnetically suspended trains, [electrical energy storage], big bubble chambers for nuclear research, as well as going in an MRI […]. But the actual development took a long time.

BROCK: I'm sure.

TANENBAUM: And whether very much of the royalties was collected or not, I really don't know.

BROCK: Well, I would like to <**T: 15 min**>…I don't know where Christophe…I have an interest in going back and talking a little bit more about the Shockley diode effort…

LÉCUYER: Absolutely, that'd be great.

BROCK: …as a place to go. Now this was a device suggested by Shockley, I take it?

TANENBAUM: Yes. I don't know whether he had the…whether he was the original…who recognized that that kind of a device would have that characteristic. That I just don't know.

BROCK: Oh, okay.

TANENBAUM: But it was clear even before I'd invented the diffused-base transistor, this was sort of a Holy Grail to find a solid-state cross-point as it was called, that could be used in telephony.

BROCK: Was there…I think, if my understanding is correct, there was a group going after a PNPN device at the same time that you were going after the silicon transistor, John [L.] Moll's group…

TANENBAUM: John Moll. Yes. John and I collaborated on making the first PNPN. Because John came over…John is an electrical engineer, basically, and asked whether you could make a PNPN device. I said, "Well, let's talk to Cal Fuller and see if he can diffuse a PNPN structure." John had an idea, not just…in fact, I think John's interest was not so much in the PNPN. He was interested in the fact you could also make a thyristor out of that, a device [widely used to] control power.

BROCK: Right.

TANENBAUM: So we got the stuff…we got the material from Cal. I made the contacts to it, and John measured it, and…

BROCK: Okay.

TANENBAUM: …and we did write a paper on [it].[9]

BROCK: So that was I think…am I right, that's in like the 1956 timeframe, I think…?

TANENBAUM: By the time the paper was published, that's very likely.

BROCK: Yes, 1956. Yes.

TANENBAUM: Yes.

BROCK: Was…oh, I'm sorry.

LÉCUYER: Was Moll in the Shockley group or was he…?

TANENBAUM: No, he was over in the development area. He was in Morton's group.

LÉCUYER: Okay. Okay.

BROCK: And did you…you recognized the importance of that device as the switching element. I mean that's the…yes. Well then, I guess it was in '55 that, also, Carl Frosch comes up with this invention/idea/development [that] the silicon dioxide layer could be a diffusion mask. What was the reaction like to that development? Was that after you had created your transistors? Or…?

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[9] J.L. Moll, M. Tanenbaum, J.M. Goldey, and N. Holonyak, "P-N-P-N Transistor Switches," *Proceedings of the IRE* 44(9) (1956): 1174-82.

TANENBAUM: You know, it was around the same time. I don't recall if it was afterwards or before. It was a kind of serendipitous discovery. He was diffusing…doing diffusion in silicon for solar cells. He observed the symmetry that he got in terms of diffusion depending upon whether the…essentially, the thickness of the oxide layer, and that was very useful in making solar cells. Protect…when you're doing your heavy diffusion on one side, you can protect the other side from it. That was thought to be, essentially, the first application.

It was also just about the same time that Jules Andrus, who was, I think, a technician…I'm not sure. I think he was a technician. He may have been a member of the technical staff. But he was…and he was over in the development department. He [started looking at] photolithography [to etch patterns in the silicon dioxide]. I guess it had to be after Frosch, because that's what they were really trying to do is to pattern the silicon dioxide, primarily be able to [make] mini-transistors, and not thinking about hooking them up that way, unfortunately.

BROCK: Was he really the author <**T: 20 min**> of bringing the sort of photolithography to bear on the silicon semiconductor work?

TANENBAUM: […] I don't [know if] it was his idea. I don't know whose idea it was. But I think he was [following the suggestions of others]…

BROCK: Okay.

TANENBAUM: …to see what kind of resist you could get, and what kind of definition you could get, and the kind of protection you could get.

BROCK: But it was a Bell Labs…

TANENBAUM: Yes.

BROCK: …idea.

TANENBAUM: Yes, I think so. I think so. I don't know that…it's been my…I don't think anyone else was working on that at the time.

BROCK: But that was, again, from the development side of the street. And you were over here in research. Well, I guess I was going to move on to talk about […] Moll's transistor. Is that okay with you Christophe? I was interested to see reference to, also in '55, later than your work, Moll produced a diffused-base mesa transistor using the oxide layer. Are you familiar with the context of him doing that work?

TANENBAUM: That's altogether possible, but I don't remember that.

BROCK: And he was on the development side…

TANENBAUM: Yes.

BROCK: …as well, so maybe it's part of that divide.

TANENBAUM: Certainly the term "mesa transistor" is familiar.

BROCK: Right. Well then, maybe we can move on to discussing the story of Shockley's departure, would that be okay? You know he…around the time you're having your great success, you know, Shockley's out and establishing Shockley Semiconductor [Laboratory] as a subsidiary of Beckman Instruments. What was the reaction of people working in semiconductors here at Bell Labs to Shockley's departure?

TANENBAUM: I think it was not unexpected. I think there was somewhat of a problem of Shockley's recruiting [Bell Labs people]. He offered me a job, for example, for twice my salary. I sat down and had a heart-to-heart with my supervisor and decided I liked what I was doing here at Bell Labs. But I could have been a billionaire.

BROCK: Well, could you tell us a little bit more about how you're thinking…you know the story of his attempt to recruit you? Because I found it interesting that apparently he tried to recruit a number of people, all of whom refused, which I find that uniformity interesting.

TANENBAUM: But not all, because Bob Noyce had accepted an offer here, but he never really…he never showed up.

BROCK: I didn't realize that.

TANENBAUM: Yes.

LÉCUYER: So he was offered a job by Shockley here…

TANENBAUM: Yes, I think so. It was for Shockley's group here […].

BROCK: […] Well, did he phone you up or what was the…?

TANENBAUM: Well, I…you know, I don't remember whether he called me. I suspect he called me, because I think it was well known that he was leaving and going to Beckman at that time. So, I suspect it was a phone call.

BROCK: Right. Why didn't you accept? Why did you…?

TANENBAUM: I liked what I was doing too well. I say I had…I was not concerned about the relationship, because I had gotten along just fine with Bill. But I liked what I was doing, and my family, my wife's family, were on the East Coast, and we didn't need to be going to California. I had a sister out there, but, no, that was the final basis, the fact that I liked what I was doing here. Everybody told me that I had a future here.

BROCK: What were…?

TANENBAUM: I got a raise.

BROCK: Oh, good. I didn't want to…

TANENBAUM: Not until after I made up my mind.

BROCK: I was curious about that <**T: 25 min**>, but I didn't know if I should ask it or not, if that was a good lever for a raise. Were there discussions here amongst the other folks who Shockley was making offers to…?

TANENBAUM: I didn't…I didn't talk to anyone else.

BROCK: What did you think of his departure, not just that he left, but his departure to set up his own firm, aiming toward, I think their initial aim was toward, you know, the mass production of silicon transistors. What did you…?

TANENBAUM: That was actually the mass production of PNPNs, the silicon cross-points. In fact, it's my understand—and I think this is right—that Noyce and his colleagues came to understand, first of all, this is a difficult device to make because the control required for cross-point application. Second of all, that there was a really growing market for silicon transistors, and they wanted to move in that direction and Shockley would not move. [I understand] that's why they left and went to Fairchild [Camera and Instrument Corporation], and got funding from Fairchild.

BROCK: Did your…I guess in the lifetime of Shockley Semiconductor, before it was acquired by [Clevite], whatever it was, was basically '55, '56 through 1961, I think. During that time, did you have interactions with Shockley and his group on the silicon work?

TANENBAUM: Well, we had interactions right at the very beginning, of course, with the…I mentioned Gordon Moore…

BROCK: Right.

TANENBAUM: But after that, of course, I was kind of out of the silicon business after that. I certainly had no interactions with Shockley. [Shockley was a patent licensee and he sent Gordon Moore, whom he had just hired, to talk to me about the diffused-base transistor. Later on, after Noyce and his colleagues had left Shockley], I was [in California] visiting my sister, who lived in Palo Alto. Her husband worked for Lockheed [Martin Corporation] out there. I'm trying to remember whether I called Bob Noyce, or whether he learned I was out there. I think I must have called him to say, "I'm out here. What are you guys doing?"

He invited me over and he showed me what had to be one of the very earliest integrated circuits […]. That looked very interesting to me, but I could not extrapolate it into what eventually was going to happen. But that's about the only interaction I had with that group.

BROCK: Did you meet Noyce in the context of Shockley hiring…you know, making him the offer to come here to Bell Labs…?

TANENBAUM: Yes. I met him when he was here interviewing.

BROCK: What was your impression of him?

TANENBAUM: Oh, I thought he was a very bright guy.

BROCK: Did you have interactions besides that visit to California with your sister? Did [you] have any other interactions with him over time, you know, because you…?

TANENBAUM: Oh, I would see him on occasion, and I was a member, then an officer, of the National Academy of Engineering. I'd see him there occasionally, and I was there when the prize…because the [Charles Stark] Draper Prize was awarded. But no, our paths…we didn't really cross very often.

BROCK: What about Moore? Did you see him again after he came by for his quick…

TANENBAUM: Oh, I…

BROCK: …tutorial?

TANENBAUM: I think probably similarly, yes. But nothing that stands out in my mind.

BROCK: Was it working on the PNPN project, then, that was essentially your last project in the silicon work?

TANENBAUM: Yes.

BROCK: Yes.

TANENBAUM: Oh, other than the radiation stuff that I [undertook].

BROCK: Right. Well, there's…it seems like you had a couple papers…

TANENBAUM: Oh, I did. I did have…

BROCK: …on…

TANENBAUM: …one other…. Again, this was…and I'm trying to remember where was at the time. [There was another encounter with silicon when] I was in the metallurgy department. We had a visit from DuPont, who came in <**T: 30 min**> and showed us a silicon crystal, single crystal. I don't know, maybe a foot long and a good fraction, large fraction, of an inch in diameter showing the [crystal facets and…] which [was] grown by vapor phase deposition. They had…thought they had a new way to grow silicon single…

[END OF AUDIO, FILE 1.6]

TANENBAUM: So I suggested that we get them together with some of the development people to see whether it would be of interest. Well, they took one look at it, and said, "You don't want to use that to grow silicon crystal. You wanted to use [it to grow] layers of silicon, high purity silicon on [doped silicon substrates]." That way we can make higher frequency transistors]. PNIP, which is a…silicon transfers call it a PNP. But you put a very…and this was known theoretically, but no one had ever been very successful in doing it. [If] you put an insulating layer of silicon between the base and the collector, [and], by separating them, cut down on the capacitance at that junction, [that] then enables [this transistor to operate at] much higher [frequencies]. That's [how the] epitaxy [process evolved…].

BROCK: Okay. Okay.

TANENBAUM: So I was [just] a middleman in [in that process and] I don't think I had any other real interaction with the silicon business.

[Oh, yes! There was one more as a follow-up to the epitaxy process. The desired epitaxial layers were very thin and it would be very important to measure and control those thicknesses. It occurred to me that since pure silicon is transparent to infrared greater than 1.2 microns in wavelength and the silicon substrate below the layer was highly doped and reflective of infrared, one could use an infrared interference method as a nondestructive thickness measurement. I discussed this with Bill Spitzer, who was an excellent infrared spectroscopist. He did the experimental work and we got a patent[10] and published a paper on the method].[11]

I did come back to work for Jack Morton, later on, in the development department as director of what was called, I think, the Solid-State Device Laboratory, which was responsible for all kinds of other solid-state devices, except semiconductor. So it [included] magnetic memories and microwave isolators and other ferrites…

BROCK: Okay.

TANENBAUM: …and that's where the…that was the group that did [much] of the solid-state laser work at Bell Labs.

BROCK: Right. Christophe did you have some questions here, or…?

LÉCUYER: No, not particularly.

BROCK: Okay. Well, what was…going to say, oh, yes. I guess it was in '56 that the Labs hosted the third transistor symposium, where I assume a lot of this silicon work got disclosed to the licensees. Did you have a participation in that?

TANENBAUM: You say the third…

BROCK: Or perhaps I'm mistaken…

TANENBAUM: I don't know. I don't know. It might have been. I thought it was the second.

BROCK: Perhaps it is. Is that right, Christophe?

LÉCUYER: I think so.

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[10] See U.S. Patent 3,099,579, issued 30 June 1963.
[11] W.G Spitzer and M. Tanenbaum, "Interference Method for Measuring the Thickness of Epitaxially Grown Films," *Journal of Applied Physics* 32**(**4) (1961): 744-5.

TANENBAUM: Yes.

BROCK: I'm probably wrong then.

TANENBAUM: Assuming that was the second, yes. Yes, I was an active presenter in that.

BROCK: I was just interested: about how many firms were represented there? Because to me, it strikes me as a very important event for just…

TANENBAUM: Well…

BROCK: …diffusing the silicon technology.

TANENBAUM: Yes. It was…my recollection is that the auditorium here, Fletcher Auditorium—I guess it's still called the Fletcher Auditorium—was full, and its capacity is probably someplace between one and two hundred, probably more than a hundred, I don't think as much as two hundred. No, it was…my recollection is [that] it was very well attended. But I think all of our licensees were represented, some by several people.

BROCK: By this time had there…had any sort of sense of a community of people working with silicon transistors developed? I mean, was it everybody in that room, or was it really the people here at Bell Labs who formed this silicon community? Do you get the sense of the question…?

TANENBAUM: Yes. My impression and recollection is that most everybody moved from—who was in the semiconductor business—moved from germanium to silicon fairly quickly. You know it wasn't that easy a move, because you had…back in those early days you had to grow your own silicon crystals. Others…I guess [later], TI went into business of selling silicon crystals. I think I could identify Monsanto at some point in time, also in that business. But in those early days, there was not a lot [of silicon crystal around], so people had to <**T: 05 min**> more or less grow their own from scratch. Now very…I understand very few of the semiconductor manufacturers grow their own crystals. They buy crystals from people who make a business growing great big and monstrous…

BROCK: Yes.

TANENBAUM: To the degree, the business was really pretty highly competitive. We were probably the only noncompetitive part of the business. So there was an awful lot of [technical] exchange between us and all of our licensees. I have no…and then there were things like this IRE device conference and, of course, the regular meetings, the on-the-record meetings, picked up very, very quickly in that area. So certainly there was a community of people working in silicon, but how much interaction there was between them, I don't really know.

BROCK: Well, it was in…I guess it was in September '56 in the proceedings of the IRE that the paper on the PNPN transistor switches comes out.[12] There's one person who was involved in that work, but I don't think we discussed before, that's Goldey.

TANENBAUM: Jim [James M.] Goldey.

BROCK: Yes. Could you tell us a little bit about him and his contributions to that work?

TANENBAUM: I'm afraid not.

BROCK: Okay.

TANENBAUM: I knew Jim. We were…Jim's a very friendly [and capable] guy. [I believe he was working with John Moll.] But I sort of moved out of the area. Jim had not been around all that very long at that stage of the game. I don't [remember] any [direct] collaboration while I was involved in that.

BROCK: Okay. Well, we did discuss before how the Shockley diodes were such a divisive effort at Shockley Semiconductor leading very directly in a way, to the formation of Fairchild. We were curious to know if you had any reaction to this 1956 paper from Shockley, you know being able to succeed where they had not.[13]

TANENBAUM: Well, no, I think we had demonstrated the PNPN prior to Shockley's actual paper.

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[12] Ibid.

[13] Chih-Tang Sah, R.N. Noyce, and W. Shockley, "Carrier Generation and Recombination in P-N Junctions and P-N Junction Characteristics," *Proceedings of the IRE* 45(9) (1957): 1228-43.

BROCK: Oh, maybe that's just a publication date, right.

TANENBAUM: Yes. So [I understand that] he set up, primarily, to capitalize on the four-layer device.

BROCK: On what you had achieved here. Yes.

TANENBAUM: Yes.

BROCK: Okay, that makes a lot of sense. So, it would have been an encouraging development. Okay. Yes. Then it seems in '58 is when you have these series of papers on—'58, '59—of different ways to get uniform P-type or N-type silicon, one on using zone leveling for the P-type silicon.[14] Then I think, it's later with the N-type, and that was '61 for the N-type silicon by the nuclear transmutation. But as you're moving into the metallurgy section, there seems some very interesting work by a [Martin M.] Atalla and [Dawon] Kahng in the 1958 to 1960 period working in silicon, you know creating the first MOS [metal-oxide semiconductor] field effect…

TANENBAUM: Effect …

BROCK: …transistor. Could you tell us a little bit about your experience of those individuals, and the reception to their work here?

TANENBAUM: Not really. I'm really not in a good enough position to do that. You know, I heard about it, and it was interesting. That's the structure that [John] Bardene, [Walter H.] Brattain, <**T: 10 min**> and Shockley were trying to make when they discovered the transistor effect. So it was kind of nice to see it finally come to something. And in many ways it's much more important than the junction transistor…today.

BROCK: Right. Well, let's see. Just staying in this period of maybe '57 to '60, it seems like there's a lot of departures from Bell…of Bell Labs' staff to other centers in the developing silicon semiconductor work, you know, be that academe—I guess, Pearson and Moll go out to Stanford—other people are going to HP [Hewlett-Packard Company], or TI, or Motorola, [Inc.],

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[14] E.D. Kolb and M. Tanenbaum, "Uniform Resistivity P-type Silicon by Zone Leveling," *Journal of the Electrochemical Society* 106(7) (1959): 597-9; and Tanenbaum and Mills, "Preparation of Uniform Resistivity N-type Silicon by Nuclear Transmutation."

presumably. Was there…or what did people here make of that? Was that the normal course of sort of the evaporation rate, or was it, you know, seen like an exodus?

TANENBAUM: No, it wasn't anything particular. It wasn't anything desirable, but nothing particularly unexpected. They did a similar movement shortly after the invention of the transistor, I am told.

BROCK: Oh, yes.

TANENBAUM: Yes. People…one set up Transitron [Electronic Corporation], I guess is one name that comes to mind. In fact, the feeling here was that—what was his name, Batchelor or something like that, at Transitron [David Bakalar]. One of them…two brothers, one of them had come here to get a job to learn how to make germanium transistors, or diodes I think it probably was, with the idea that they were going to set up a business. So the relationships with that company were never very strong.

But, no, that, I think, was happening all around. You know a brand new industry was growing, lots of opportunities, [and] to a large degree were successful: that's where the money was…

BROCK: Right.

LÉCUYER: Les [Lester C.] Hogan, left as well, right?

TANENBAUM: Yes.

LÉCUYER: I mean he left around the time when you arrived here, right?

TANENBAUM: Well, it was a little while after that…

LÉCUYER: I see. Okay.

TANENBAUM: Les…if I hadn't gone to work with Joe Burton, I would have gone to work with Les.

LÉCUYER: I see.

TANENBAUM: I even forgot what it was he was doing. But it was interesting.

LÉCUYER: Okay. And then he moved to Harvard [University], right?

TANENBAUM: I really lost track of Les […]. When I left silicon, I left silicon.

LÉCUYER: Okay. Okay.

BROCK: Who were—in the mid '50s—who were the main chemists working in silicon on the development side?

TANENBAUM: For the most part, I don't…I'm not even sure about the backgrounds of people like Jim Goldey. John Moll, I know, was great, [but] not a chemist.

BROCK: Right.

TANENBAUM: Jim Goldey, I'm just not sure. He might have been a physicist. I don't think…for some reason I don't think he was an engineer <**T: 15 min**>. But I'm not even sure of that. Only one person comes to mind, but I can't think of her first name, but her last name was [Eileen] Tannenbaum. She spelled it with one more "N" than I use. She did some very interesting work on the silicon oxide, silicon interface. I don't…I really don't know. I haven't seen or heard of her for some time.

LÉCUYER: So when was that? In late '50s?

TANENBAUM: I would think that was probably the late 1950s. Yes.

BROCK: Well, it would be interesting to find a good person to interview from…who worked in the development group on the silicon. So, maybe we can talk about that another time, who a good informant might be there.

TANENBAUM: Iverson. Eric Iverson.

BROCK: Eric Iverson.

TANENBAUM: In fact, I think he may have been another Johns Hopkins background. I have no idea where he is.

BROCK: We can do something.

TANENBAUM: Most of the people over there were either physicists or electrical engineers who learned chemistry. But Eric Iverson, I think, was a chemical…PhD chemical engineer, but I am not certain of that.

BROCK: We'll check it out. So in this period of '57 to '60, as the semiconductor industry is expanding with all these firms opening up shop on the West Coast—Fairchild, Hughes Semiconductor [Hughes Aircraft Company], Pacific Semiconductor[s, Inc.]. Do you have recollections about the Bell Lab staff providing advice to these upstart firms on the West Coast?

TANENBAUM: Well, I'm sure just as a matter of policy. They were licensees, our licensees. They were welcome here. We talked to them about just about anything that was going on as long as it was properly protected.

BROCK: Right. But there was, for the licensees, they could ring you up or arrange for a visit…

TANENBAUM: Visit…

BROCK: …and as long as you had the patent coverage…

TANENBAUM: Sure. Sure.

BROCK: You were encouraged to disclose it all?

TANENBAUM: Sure.

BROCK: Well, Christophe did you want to ask any other questions on the planar process, or integrated circuit area?

LÉCUYER: Probably not.

BROCK: No, okay. Okay, good. Great. Well, we talked a little bit more…a little bit about your roles as sort of a broker in the development of epitaxy work. In some of my research, I saw Henry [C.] Theurer, if I'm pronouncing correctly…

TANENBAUM: You are..

BROCK: …Theurer. Coming up as really developing the epitaxy work for silicon, does that…

TANENBAUM: I…

BROCK: …sound familiar?

TANENBAUM: I associate Henry with the floating zone refining.

BROCK: Right.

TANENBAUM: He's the one who really did that. But I don't…you know, I don't recall him working in epitaxy.

BROCK: Who do you associate with those developments?

TANENBAUM: Friedolph Schmit was one of the guys who was in that first group that we got together. It was all the epitaxy advantages […]. Jim Goldey may very well have been one of them, but I don't remember. But I do very clearly remember Friedolph. But I don't really associate Henry with that, but that doesn't mean that he wasn't. It doesn't mean he wasn't doing that.

BROCK: Well maybe, then, we could move to talking about…I mean we've talked about the superconductivity work. I think it was soon after, I mean very quickly that they achieved such amazing success with the superconducting magnets. You co-wrote an article in *Scientific American* <**T: 20 min**> in 1962 on the superconducting magnet work.[15] We talked a little bit about the trajectory of that work here. It seems right thereafter was when you, in the 1962 time period, is when you made the move to development and that solid-state development laboratory. Maybe, could you tell us a little bit more about—well, you talked in brief about that laboratory—but could you tell us more about that next step in taking wider management, sort of, responsibility?

TANENBAUM: I accepted that job with some trepidation, actually, because I didn't feel like I knew that much about electronics and electronic devices. I knew a lot about transistors, but this was everything but transistors. That really was the definition of the job. So, it was masers and then, later, lasers and magnetic memories, and microwave devices using ferrites, isolators, and things of that sort. Prototype device microwave isolator that was used in Telstar was built in that laboratory at that time. But I came over there with…knowing that I had a lot of learning to do, and I did. I learned a lot.

The people there were, for the most part, either chemical…electrical engineers or physicists' background. Derrick [Henry E.D.] Scovil was a physicist, too, who did most of the work on masers, and then lasers. The first YAG [yttrium-aluminum-garnet] lasers were built in that laboratory, the crystals were grown by [L.G.] Van Uitert. Is that a familiar name to you?

BROCK: Vaguely.

TANENBAUM: Yes. He was very, very interesting guy. I think he died just a few years ago. But he was the molten salt specialist, to start with. He would grow…he grew the titanate crystals and all kinds of ferrite single crystals that were…he was a very, very popular guy here among the applied physicists. If you wanted a crystal, Van would figure out a way to grow it. But he produced the first YAG crystals. Then, Derrick Scovil laid the first…Scovil and [J.E.] Geusic, I think, made the first…laid the first YAG lasers in there. It was a very busy, very exciting time.

It was also the time when the possibility of making semiconductor memories was first demonstrated. I don't know that that was […] done here, but using the MOS technology…because I had a group that was working on magnetic memories, and so the real

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[15] J. E. Kunzler and M. Tanenbaum, "Superconducting Magnets," *Scientific American* 206(6) (1962): 60-7.

question was: are semiconductors really going to make it? You know, they're volatile. You turn the power off and the memory's gone. But people have solved that problem pretty well. But those are the principal things that come to mind. I was there, I guess, for just a couple of years.

BROCK: Yes, […] '64. Sixty-two to '64. How big of a group was it? Was it also here on the campus?

TANENBAUM: Yes. Yes. It was in the […] building, which is where the transistor work [and] physics department [were] also, although my first lab was just in this building, in the chemistry section. But the group, I guess, was typical. I don't remember exactly. Typically, there'd be three or four sub-departments [in a] department—I think they were probably called departments by then, each one with ten, a dozen, professionals in it. So [my department] was probably a group of forty professionals or so, and equivalent number of technicians.

BROCK: And being on the development side, was it your task to develop devices/technologies up to the point where they could be <**T: 25 min**> transferred to one of the manufacturing facilities?

TANENBAUM: No, not really. This was what we called "exploratory development." It was looking for new device concepts, essentially. Then studying them enough so that you could determine how they could be exploited. Then you'd hand it over to a final design group. That was, at that time…typically, it would go to a place, one of the branch laboratories. But it might also go to some of the development laboratories here. The final development—where the screw goes, what size it is—was done principally at the branch laboratories.

BROCK: Was there an evaluation process before some of these things, like, oh, I don't know, the lasers left the experimental development…now what was the…what would you call it? The gating process or decision-making process to see if it would then go the next step of development. What was that like?

TANENBAUM: Well, it was a decision made at various levels, depending upon the size of the project.

BROCK: Oh, right.

TANENBAUM: The vice president for component development, Jack Morton, of course, was involved in the final decision for a brand new line of devices. But it was more than just the mode of development, because you weren't going to manufacture it, and that's when to use the system. There were councils that were set up here at the Laboratories. There was a Switching Council and a Transmission Council, in particular. I think those were the two principal ones, because they're the ones and…no, there had to be one also in customer premise equipment. […] They were the ones who [recommended] what components were going to be used, what systems were going to be made and what components were going to be used. So they would be involved with it.

But also, AT&T [(American Telephone and Telegraph Company) was the last word]. The Engineering Division of AT&T was the one that finally said, "This is what Western Electric…we're going to recommend Western Electric make for the telephone [companies]." Western Electric, of course, had to make the…had to agree to that decision. So these councils, the Switching and Transmission Councils, each had Western Electric members, AT&T members, and Bell Laboratory members. That's where you would decide, "Okay, we're going to make electronic switching systems." That would, of course…you'd have to have the agreement decision of just exactly what you were going to use in the component areas, as well as all the other stuff it takes to put a major system out in the field.

LÉCUYER: How often did they meet?

TANENBAUM: At least quarterly, at least quarterly. I'm trying…I served on both of those councils at one time or another. I'm trying to remember, but if you had a decision to make, you usually had a special meeting. For something big like a switching system, you'd take one of our large conference rooms and fill it up with people. The decision was pretty much made by then, but this was the final formal agreement.

LÉCUYER: Okay.

BROCK: What did you do as the director of that laboratory? I mean what was your…how did you direct? What were your practices? What was a day like for you?

TANENBAUM: You'd…most days you'd spend time walking around and talking to people. There were certain projects going on, so you'd have periodic reviews of those in a more formal sort of way. But it was mostly kind of one-to-one interaction with your technical staff. There was a process here—I don't know whether it still exists—but when you worked, your work usually was associated with a particular development case or a research case. Once a year, each [member of the] technical staff, particularly in the research <**T: 30 min**> department where everybody had pretty much their own thing…you'd write up a short report, and I mean a short report—a page or two—in terms of what you've done [in the past year], what you're proposing to do for the coming year. Now you get into a system development or what have you, they're much more formal, much more detailed, and much, more lengthy. But…and those cases would go to Western Electric and AT&T for their review and approval, because that…

[END OF AUDIO, FILE 1.7]

BROCK: Okay.

TANENBAUM: …what I would call in the bureaucratic procedures that an organization requires to keep its records. But for the most part, it was a collegial kind of interaction in the jobs that I had.

BROCK: Were you in those discussions helping them solve problems of whatever kind, or encouraging…

TANENBAUM: Define…

BROCK: …associations between people, or…?

TANENBAUM: Yes. One of the…I always felt that one of the principal responsibilities of research management, in particular, is to be as aware as you can be of what the development people need, what they're looking for. So that…and to know exactly what the people you're responsible for, the work they are doing, what they are finding out. Both to assure yourself that your work is in areas that have some chance, some reasonable possibility, of finding applications, and you served as principal responsibility of research management—as far as I was concerned—to make sure that those bridges existed. [I had] the interface [responsibility]. Now, you…if some of your people doing research developed the relationship with someone in the development department that was terrific. But it didn't happen very often unless you could help make that connection.

In Exploratory Development, where I was in my last job [at the Labs], it was then the question between exploratory development of research on the one hand, whether there's some new possibilities for devices coming out of there, and the final development people who knew what the systems people wanted.

BROCK: Okay. So it was a translator connection.

TANENBAUM: Yes.

BROCK: Was this…I was just wondering about the…

TANENBAUM: That, by the way, is in conjunction with a number of other things, such as deciding who really isn't able to do the kind of work that you expect of your people, and, also, recruiting in terms of hiring the people.

BROCK: Was there a formal or informal process of acculturation for people going from a member of the technical staff to research or development management position? Was there any sort of management training? Or…how did that work?

TANENBAUM: Well, management training was in its infancy when I was [there]. There were some courses that were started. I remember one called "Talking with People," where what you learned was [that] the important thing is listening to people, not talking to them. Getting them to talk to you, essentially, is it. So there…I'm sure there's a good deal more management training now, but there was very little, very little when I was here forty-something years ago.

LÉCUYER: So it was really training by practice.

TANENBAUM: That's right, and watching…

LÉCUYER: Looking at other people.

TANENBAUM: Right. You start off working for a manager, you sort of get some feel of what that interaction is and should be, at least as far as you're concerned.

LÉCUYER: Was there mentorship for the people…?

TANENBAUM: Yes, but no formal process. For example, if Gerald Pearson had not come over to me and said, "I think maybe you can make some interesting magnetoresistance measurements," I wouldn't have thought of it. No, it was…looking back on it, I think it just…had an extraordinary atmosphere, and particularly for young people just starting out. For the most part. There were exceptions: you run into some people who were pretty secretive about it. They didn't want to talk about what they were doing, until they'd done it. But for the most part, people were very, very open. You could…no trouble getting to see anybody you wanted to see <**T: 05 min**>.

BROCK: Well, I think we've talked a bit about Jack Morton, but we haven't talked directly about him, as an individual. Perhaps, you can tell us a little bit what it was like…what he was like working with on that side of it?

TANENBAUM: He was very, very driven man, is what I'd say. He expected you to really know what was going on in your field. He expected you to be the best: "My people are going to be the best." There was a newspaper…there was a trade publication, *Electronic News*. I don't know whether it's still around or not…

BROCK: I'm not sure.

TANENBAUM: …but when you got your copy of *Electronic News*, you opened it up quickly because you knew Jack was reading it. If something had happened in your field, you better know about it. He was a very, very hands-on manager, but not restrictive in any way. I have to be careful about that, when I say that, because he would make up his mind on a given direction, like the silicon direction. He'd say, "We're going for silicon. You've got something that has to be finished up with germanium, go ahead and do it, but we're going to silicon for now."

In fact, I think that was, in a sense, one of Jack's…Jack's weaknesses. He worried about the future of integrated circuitry. He wasn't sure that you could get the control, the dimensional control and what have you, that you need. One of the inventions here at Bell Labs was something called "beam lead technology," where, essentially, you created your devices [such as] a transistor, and built up electric plated leads [that could be] etched [out]. So you could now mount that tiny transistor wherever you wanted. Use a ceramic substrate [to…] build capacitors, and what have you. He called that the "right scale" of integration.

So there was a good deal of frustration, I know, in the…I wasn't personally involved in that at all, but I know that there was a good deal of frustration in the group of people who felt [silicon, fully] integrated circuitry was really the way to go. I think maybe that's one reason why some of the inventions that could have and should have been made here were not made here.

LÉCUYER: This would that have been 1960, '61, '62?

TANENBAUM: It'd be in that period. Yes, I think that's right. But he was very strongly driven. I would say that's the only area I can really think of where his leadership was overly strong.

LÉCUYER: So if you could go back a bit in the past, about what we talked about before, I have the feeling that by '56 or so all the silicon work had moved to development.

TANENBAUM: All of the device work…

LÉCUYER: The device work.

TANENBAUM: Yes. Yes.

LÉCUYER: Yes, okay. So there was only materials work what you were doing…?

TANENBAUM: Yes. I had…and there was still [experimental and] theoretical studies, theoretical physicists interested in silicon.

LÉCUYER: Okay. Why do you think that this move…why was such a move made at that time?

TANENBAUM: Because it…at that time, it wasn't really considered research anymore.

LÉCUYER: I see. Okay.

TANENBAUM: And there wasn't…and there wasn't a group with the responsibility for the next step to take it.

LÉCUYER: Okay. Thank you.

TANENBAUM: If I wanted to continue working on silicon devices, I'm sure I could have.

LÉCUYER: When you moved…?

TANENBAUM: Yes. I don't think I would have, anyway. I think I would have…anything that I would have done…now Carl Frosch, for example, stayed in the research department and kept working on the silicon dioxide process. But a lot of his work was done at the request, essentially, of the device department.

LÉCUYER: […] I was trying to compare the Bell Labs center, the way it was—research and the rest—with the Fairchild center on the same time. I have a feeling that the borders <**T: 10 min**> were placed at different places.

BROCK: Oh, between research and development…

LÉCUYER: Between research and development.

TANENBAUM: Oh, Fairchild never did what I would call research.

LÉCUYER: Oh, I see. Okay. Okay.

TANENBAUM: They did what I would call…we would have called "exploratory development," looking for new device structures rather than trying to [discover] something that had never been [known].

LÉCUYER: Okay. Okay.

TANENBAUM: You know the work I did here could have been done in the development department. They were trying to do it in the development department. I can only assume that what motivated Shockley was he just didn't think it was being done fast enough, well enough, whatever.

LÉCUYER: Yes, okay.

BROCK: Well, maybe let's pause here for a moment, because…

[END OF AUDIO, FILE 1.8]

[END OF INTERVIEW]

INTERVIEWEE: **Morris Tanenbaum**

INTERVIEWER: **David C. Brock**

LOCATION: **Bell Telephone Laboratories, Inc.**
Murray Hill, New Jersey

DATE: **26 July 2004**

BROCK: [This is a] continuation of an oral history interview with Morris Tanenbaum, conducted by David Brock, meeting at Bell Labs on July the 26th, 2004. As I mentioned, I thought we would return to the period of '62 to '64, when you were acting as director of the solid-state development laboratory and the electronic components divisions here at Bell Labs, and to ask you about this period in the early '60s coming off of your work on the silicon transistor, to ask you about if and how you were continuing to follow developments in silicon semiconductor technology.

TANENBAUM: The answer to that question is yes. Primarily because some of the work going in exploratory development, for which I was responsible, was competitive with what was going on in the semiconductor world. For example, we had responsibility for doing exploratory work for memory other than semiconductor memory and so such things as thin film, magnetic materials, ferrite cores and ferrites, various other technologies of that sort. So that was really to do…we were watching very carefully what was going on in integrated circuits, and the possibility of integrated circuit memory, which, of course, was the eventual winner of that race.

The other work that was going on in the department was varied. There was work on microwave ferrites for isolators and…primarily isolators. The microwave ferrite devices that went into the first telecommunications satellite, Telstar, [were] developed in that lab. But I would guess that, probably, the more interesting work to me was the work on solid-state lasers. We had a group that did most of the early work on YAG (yttrium iron garnet) with various building materials in it. Those crystals were grown over in the Materials…I guess, it was still called Materials Science or Metallurgy Department at Bell Labs. But all of the device work was done in [our] group. So those were, kind of, the general areas of responsibility.

BROCK: In what way did you follow developments, these competitive developments, in the semiconductor area? Did you continue to attend meetings and conferences?

TANENBAUM: Sure, yes, sure. And just informal discussions with people in the silicon development area.

BROCK: Here?

TANENBAUM: Here in Bell Labs. I personally did not go to semiconductor conferences, but people in my department tracked those very, very closely. You know the work on integrated circuitry in Bell Labs was, I think, somewhat delayed, primarily—at least the old semiconductor work—primarily because of the rather strong opinions of our vice president then, Jack Morton, who was concerned about the ability to really make large numbers of transistors on single chips, because of contamination and defect density and what have you.

So he pushed his area in the direction of what he called "right scale" integration, which used a combination of tantalum thin film and metal conductors—tantalum thin film for capacitors in particular, and [onto] which were bonded silicon chips [that] contained the active elements, but primarily as a single transistor silicon chip with what were called "beam leads." These were…essentially you made many single silicon transistors on a single slice, and then, electrochemically <**T: 05 min**> or by evaporation, you would build up fairly thick leads to the silicon chips. Then you protect [the leads] and etch them apart, cut the…

BROCK: I see.

TANENBAUM: …and so you etch away the silicon underneath the conductor. Then you would compression bond that conductor to the ceramic substrate. So the first tone generators for telephone sets were made that way, essentially. They were combinations of…they were on high purity aluminum oxide substrates with tantalum capacitors and evaporated leads. So they were an integrated circuit kind of thing, but using essentially three components: ceramics, metals, and semiconductors. That was pushed very hard by Morton. There were…I'm sure there were a few people working on all-silicon things, but he was convinced that that was going to be the way to make circuitry for telephone use.

BROCK: And he wasn't wholly idiosyncratic in that? I mean there were other groups in other places pursuing that same sort of hybrid…

TANENBAUM: Oh, sure.

BROCK: …integrated circuit?

TANENBAUM: Sure, that's right.

BROCK: Yes, right.

TANENBAUM: That's right. There were, instead of evaporation…I think DuPont had a series of pastes that could be printed on ceramics…metallic pastes that could be printed on ceramics. There was a lot of that kind of work going on. But that kept Bell Labs from diving directly into the integrated circuit path for quite some time.

BROCK: Once they…when other firms began to produce commercially, sort of all-silicon integrated circuits, did that shift the orientation here somewhat?

TANENBAUM: I'm sure it must have. But I think that happened after I left here and had gone to Western Electric. But I'm sure it must have, because, after a while, Bell Labs jumped on the all-silicon integrated circuit path. It became, obviously, the way to go; [others] demonstrated that you could get high yields if you did it properly.

BROCK: To go back for just one moment to the meetings…I'm interested in the meetings and conferences in the silicon and the semiconductor area that, in the…certainly, in the '50s that you attended and then later people in your group attended. I was wondering, in your estimation or in your experience, which were the really key meetings that you went to or that you wanted other people to go to?

TANENBAUM: I'm afraid I can't be very helpful there, because I transferred out of that just within a couple of years, I think, or at least silicon transistor work, and went to the metallurgy department. There the amount of work on silicon was really fairly limited because so much of it had moved over to the development area. So we were working on superconductors and magnetic metals and magnetic compounds, inorganic magnetic compounds, and materials of that sort. Then I came over to the exploratory development area, where we worked on everything that wasn't silicon.

BROCK: Right, yes.

TANENBAUM: So during those really…when the industries were forming, I was pretty much disassociated from that.

BROCK: For…. When you were involved with the metallurgy materials group, what were key meetings there? Was the Electrochemical Society one? Or…?

TANENBAUM: I would say probably *the* key meeting was the Metallurgical Society, which had developed strong interest in electronic [materials]. Also, that…you know it was more than just metals, but I think a lot of the work on ferrites and what have you were also there <**T: 10 min**>. So and much of the work in…I would say, it was probably mostly people in the chemistry department who populated the electrochemical meetings, although, depending on the subject matter, there would be people from the metallurgical department who went there too.

It was right during that period that the name was changed from Metallurgical Department" to the Materials Science Department. So you'll have to use those two words or those two titles interchangeably.

BROCK: In thinking about the informal communication network that you discussed about to keep a track on what was happening in silicon and in semiconductors, was that a fairly easy thing to do? Were people fairly free with information in those…?

TANENBAUM: Oh, it was for the most part, wide open. I think one of the great strengths of Bell Laboratories back then was that the communications between the applied science and development areas, particularly exploratory development areas, was informal and wide open.

BROCK: Did that hold true for communications with people outside of the Bell Labs organization?

TANENBAUM: Well, I think Bell Labs was probably more open than just about any other industrial laboratory I know., but we paid an awful lot of attention to intellectual property. So it wasn't until we were sure that that was well covered that we invited people in and gave papers. That produced a delay, I would say, perhaps of six months on the average, and, in fact, it was sort of a rule of thumb that the patent department had to make a decision within six months or else we would publish. Or let me put it another way, even if they made a decision, we expected to be able to publish in six months. Now you know there could be some negotiation going on there.

BROCK: What was the…was it a fairly close and collaborative relationship between the technical staff and the patent department?

TANENBAUM: Yes. Yes. They could sit…well, they considered us their clients. We were the source of their business. They had people who specialized in various areas, and you knew them, and you called them up when you thought you had something that might be of interest.

BROCK: Where were they located? Were they located nearby?

TANENBAUM: They were located right here in Murray Hill.

BROCK: Okay.

To change gears just a little bit, I wanted to ask you about three other Bell Labs alums who we have identified as other people to speak to about the general Bell Labs solid-state semiconductor story. We've spoken about some of these people before, but thought we could just discuss each of them.

TANENBAUM: Sure.

BROCK: And one of them is Morgan Sparks, who we have spoken about before. But I thought, I would be interested if you could…?

TANENBAUM: Morgan must be approaching ninety now. He was in fine shape when I last saw him, which was at the…when we celebrated the fiftieth anniversary of the invention of the transistor. That was '97 or so. But if you want to talk to him, I think it's a contact you ought to make soon.

BROCK: Yes. We're working on that now. How would you characterize his overall contribution here and to the field?

TANENBAUM: Well, I think he was one of the people that made the first germanium junction transistor. I think it was Sparks and Teal. He was in a…he was my boss for quite some time. […] I think he was my [sub-]department head when I first came [to the silicon work]…

BROCK: Right.

TANENBAUM: …<**T: 15 min**> and then later when I was in exploratory development, he was the executive director to whom I reported. So he was…I think he was very…and he reported to Jack Morton. He and Morton, I think, had a very close relationship. Morgan was […] very open; just a great guy to work with.

BROCK: The second person is Jim Goldey, and I know the two of you didn't overlap very much, if at all, is that correct?

TANENBAUM: Well, when I was working on the silicon transistor, Jim was around (I think he was a very recent hire then). He was in the group that Jack Morton had working on silicon, in general. I think they were trying to make silicon transistors, but they were also making other silicon devices—diodes and things of that sort. So, I knew Jim pretty well, and had very high regard for him.

BROCK: Could you characterize his key contributions that were his signature contributions?

TANENBAUM: I don't know.

BROCK: Okay, fair enough.

TANENBAUM: I don't know.

BROCK: But he was certainly deeply involved…

TANENBAUM: Yes.

BROCK: …in the efforts, especially, I guess, then on the development side?

TANENBAUM: That's right. That's right. He was in the group along [with]…I guess, I think, Nick Holonyak worked with him…

BROCK: Right.

TANENBAUM: …in that same group that had the responsibility for doing the exploratory work, exploratory development on semiconductors in particular…particularly silicon.

BROCK: And the third person I wanted to ask you about is someone I don't think we discussed last time, and that's Ian [M.] Ross. Could you tell me a little bit about your thoughts?

TANENBAUM: I think Ian came to work at Bell Labs at almost exactly the same time I did. But he went into the development department. He was…I think he got his PhD in Cambridge. Was it electrical engineering…?

BROCK: Yes.

TANENBAUM: […] Ian was, I believe, in charge of that group of…which Goldey was [a part].

BROCK: Okay. So it sounds like if we have the opportunity to interview Goldey and Ross, we'll really get a picture of what was happening over on the development side…

TANENBAUM: Right.

BROCK: …of the effort. Great.

TANENBAUM: That's right. Ian went onto become president of Bell Labs, you know?

BROCK: Right, I do. Which is an excellent comment to segue into sort of my last question in this set [which] was…a subject we really didn't get into too much last time, was about the senior leadership of Bell Labs in the late '50s and early '60s, and your interactions with that senior leadership and your impressions of that leadership.

TANENBAUM: Well, I must say that for my first four or five years, I couldn't have told you who they were. I think I heard the names. I think [Oliver E.] Buckley […] was the president of Bell Labs when I arrived. But he was very near retirement and his successor was Merv [Mervin J.] Kelly.

BROCK: Right.

TANENBAUM: Kelly's principal contribution that I knew about, technical contribution, was the…was that Kelly or was that Morton? No, that was Morton. I guess I don't know. I don't really know. But I didn't really have any…I can't recall a direct interaction with Kelly. He was a very, very interesting guy, chain-smoker. You'd see him in <**T: 20 min**> conferences, internal conferences, meetings here, sitting in the front row with a cigarette attached to his lower lip, as far as you could tell with his eyes closed. You were sure he was asleep until he asked a question. So he was quite an active leader.

He was followed by Jim [James B.] Fisk, who was the first president that I really got to know well. Jim was a, I think, MIT PhD. This institution has a longstanding relationship with MIT. I think Kelly, too, […] was from MIT. In addition to Jim…I guess the first person who was a senior executive here was Bill Baker that I got to know well. But I knew him…I got to know him when I first…he was instrumental in my recruitment from Princeton. But I think he was what we called a "sub-department" head when I first came in, although he had quite a broad reputation. I think *Fortune* published an issue back in the early '50s about promising young scientists, and he was one of the ten. As I…his work, his technical work, was in polymers and I think he was given credit for the invention of the [ablation] shield for rocketry, which is a plastic-based mixture of, I think, graphite and something else.

Also, it was…although, I don't know whether it was specifically his work, but it was at least under his direction, the replacement for lead by polymers in the sheathing of telephone cables. I think the Bell system was probably the largest…but either that or the electric battery was [the largest] consumers of lead. All the telephone cables, large cables, were lead-shielded, and that was replaced with a polyethylene plastic, which sounds fairly straightforward, but polyethylene, by itself, disintegrates under UV [ultraviolet] radiation. So the real secret there was developing a mixture that would prevent the oxidation and the UV destruction and that work was done [by Linc (Walter Lincoln) Hawkins] under Baker's leadership. [I think Linc was the first Afro-American chemist hired by Bell Labs.] That was a major, major economic contribution to the Bell system.

Let's see. Then, I guess, during my time in management at Bell Labs […]. I [already] mentioned Morton. The other person was Julius [P.] Molnar, who was the executive vice president to whom Morton reported. I think just about all the, except the [Research and Administration], vice presidents reported to Molnar. He unfortunately died, I think in his probably mid-, early- to mid- fifties, and left a big hole here. That happened after I left, but he was one of the people who I talked to when I was offered the job at Western Electric and who asked me very straightforwardly, "Why in the world would [you] want to do that?"

BROCK: Was it…well, it certainly sounds like it was the practice in that period that one would sort of come…that the senior leadership of Bell Labs would be developed from the ranks of the members of the technical staff.

TANENBAUM: I think that to the best of my knowledge that is absolutely true. I don't think anyone was ever brought in from outside for that job.

BROCK: Do you think that…I mean that must have had advantages and certain disadvantages. What do you think the advantages were?

TANENBAUM: They knew the business. They knew both the business of…AT&T's business and they also knew the R&D business. And for the most part, although I can't specifically <**T: 25 min**> identify what some of them did, most of them had significant technical accomplishments under their belt, which gave them great credibility inside the organization.

BROCK: Then maybe let's…if we go from Molnar's comment to you about…I guess it was the job that you were then offered in 1964…

TANENBAUM: Yes.

BROCK: …to leave Bell Labs to go to Western Electric, and to act as Director of Research and Development at their Engineering Research Center. Could you tell me a little bit about how that job offer arose, and your thinking about taking it, and the story of that move?

TANENBAUM: Sure. I got a call from Jack Morton who said…told me that Ray Cook from Western Electric would like to talk to me. Ray was the Vice President of Engineering at Western Electric. I knew his name; I didn't know him very well at all. He described to me this job that they had. Western Electric, I think, probably no more than two or three years before that, when a fellow named Tim [Timothy E.] Shae was the Vice President of Engineering…. Tim was a very, very interesting guy, very, very active in governmental matters, technological matters. He was very well known in Washington, [D.C.]. He was a really imaginative guy. He felt that there was a gap between what was going on in science and what was going on in manufacturing. [The offered position] was an opportunity to close that gap. Bell Labs had, really, I think, very unusually good connections between what we called final development and manufacturing, early manufacturing. There were outposts at Bell Labs located at a number of the major manufacturing organizations; we called them "branch" labs.

The one I was most familiar with was in Allentown in the semiconductor area, and there was a Bell Labs group, which, actually, had their own piece of the building right in the manufacturing building, right next to the manufacturing engineering. As new products came down the line, they would work with the manufacturing engineering to get the final changes with regard to how to make this easy to [manufacture], as well as…and still preserve the functional characteristics that you wanted [and] needed, [particularly the] reliability of the product.

So that was very, very close, but the connection with what was going on in new science was from…"new science": from science to applied science to exploratory development to initial product development. Then it went to final development at the branch labs. So there was a large gap there. Tim had the belief, which I think was right, that there were important things going on, discoveries coming out of science that could be applied to manufacturing processes and manufacturing techniques through a similar path. See, Bell Labs was product development.

BROCK: Right.

TANENBAUM: Western was responsible for introducing new technologies into manufacturing. Now, you know, there are often good connections there, but they weren't until things had gotten to the product stage that those connections really became highly active. So Tim got Western to establish this manufacturing research laboratory, whose job it was to bridge that gap, and, in particular, to look at new developments, new discoveries in science and ask the question, "What possible implication does this have on manufacturing process?" Not on product development, but on manufacturing process.

BROCK: On the process of manufacturing <**T: 30 min**> itself.

TANENBAUM: Right.

BROCK: I see. Right.

TANENBAUM: And I could give you what I think is probably the best example of that, that happened just at about the time that I came to the manufacturing…to the ERC, Engineering Research Lab, Center, we called it, which [connected] one of the latest discoveries in science [to] one of the oldest manufacturing techniques, [namely] the drawing of copper wire. In the final stages of drawing copper wire, you used diamond dies. And so what you do is take small industrial diamonds and you grind a hole in them, and polish, and that's the die.

BROCK: Okay.

TANENBAUM: Well, getting that initial hole was a very laborious process, because the only thing you can grind diamonds with is diamond dust…

[END OF AUDIO, FILE 2.1]

BROCK: Okay.

TANENBAUM: …so that's a very time-consuming, and it's a high art, as well…

BROCK: Right.

TANENBAUM: …people who know what they're doing. Well, the laser was invented and here was a concentrated source of energy that could be focused down to very small dimensions, and was just perfect for cutting that first hole in the diamond die. So that was, I think, the first in the world, the first application, of laser technology in diamond dies, [and probably in all of manufacturing]. That was just exactly the kind of thing that Tim had in mind, because without somebody looking at the problem, which is old technology…. Bell Labs wasn't doing anything, while I was there, to [adapt] the laser [to the manufacturing process…]. No one in the engineering group at Western Electric—several thousand people, you know, they kind of keep up with what was going on—but no one was really close to what was going on in the lasers. So that's what this lab was established for. I was intrigued by that. That was a hell of a good idea. I was always interested in the application of science, more so than in the fundamental understanding of the science. I think that's true of a lot of people who come to chemistry, as differentiated by people who come through physics. But that's a generalization and I have no studies on it.

BROCK: Okay.

TANENBAUM: But I was very intrigued by that, and I had…you know, ever since I made the move over to…well, I…actually, it began at the time of the first silicon transistor. I got interested in how they were going to make this thing, how they were going to be produced. So I had that general interest. I also believed that [while] Western Electric was really a first class manufacturing organization, it could use an infusion of young people with higher training and education, because so much of the things that Western did were mechanically oriented. For the most part they hired bachelor[-level] mechanical engineers; they hired metallurgists too, but most of their hiring was at the bachelor degree level.

But when I went to…I finally decided…Morton thought it was a good idea. Molnar thought it was a lousy idea. I talked to Jim Fisk, also, who was…and Bill Baker, who told me they would like me to stay at Bell Labs, but they thought it was an interesting, reasonable thing to do or consider, [and if I didn't like it, they'd welcome me back to Bell Labs]. So I found it really rather an easy decision to make.

BROCK: How did the folks on the Western Electric side know to come ask you if you were interested? How did they know you?

TANENBAUM: Through Jack Morton.

BROCK: Through Morton.

TANENBAUM: Yes. Ray Cook knew Morton, and he got my name, essentially, from Morton. Morton was interested in getting people like me into Western Electric.

BROCK: Right. So did that entail a geographic move for you?

TANENBAUM: Yes, it did, but not a major one, because we were living in Madison, New Jersey […], and the lab was in…well, it wasn't in Princeton, [New Jersey]. It was actually in Hopewell, [New Jersey], one of the communities [near] to Princeton. It was…this was before the interstate system had gotten down in that area, and it's still not down in that area well. There's still a stretch of 206, which is a tough way to get to Princeton. I don't know if you made that trip…

BROCK: I have. Yes.

TANENBAUM: They started to widen that. But at any rate, I did commute for about a month and said, "This is not going to work." So we looked for a house, and found a house to buy. I could continue…it had to be built. It was in a development down there, so I continued that <**T: 05 min**> commute for, I don't know, three or four months. Then we moved in, but it was still close enough so we retained friends under this area, and built new friendships down there.

BROCK: Was the…?

[recorder off]

TANENBAUM: We were just talking about the move to Princeton.

BROCK: Now, was the Hopewell facility a stand-alone laboratory or was it affiliated with the production…manufacturing side?

TANENBAUM: Oh, no, it was a stand-alone. The laboratory had its own building, lovely location outside of Princeton, had…I'm trying to think of the structure there. There were either two or three departments, I've forgotten. One of them was computer-oriented, looking for applications of computers in manufacturing. This was just about the time that Digital Equipment [Corporation] produced their first mini computers. I saw one of the very early ones down there. But they were looking at material: to adjust the material flows, how you manage manufacturing inventories, how you process orders, things of that sort. The rest of the work was devoted mostly to the manufacturing process itself. We developed some of the very early techniques on making optical fibers, for example. Again, one of the other applications of the laser was to use a laser beam and look at the diffraction pattern in order to maintain the [fiber] diameter […] actively, dynamically, while you were pulling the…

BROCK: Oh, right.

TANENBAUM: …[optical fiber] so that then you could build a feedback loop there. Another laser application was in these "right-scale" integrated circuits. You would print a lot of those on fairly good size pieces of aluminum [oxide] and then you had to cut them up. The laser proved to be just ideal for cutting very hard material like aluminum [oxide]. Another technique that had just been developed when I got there, and they were putting it on the factory floor…in making these "right-scale" integrated circuits, you [deposited] the tantalum, which was a highly refractory metal, [with sputtering]. To produce the thin film, you'd use a bell jar and you'd sputter it, and it was a batch process. So they developed a continuous process where you could introduce, through several stages of pumping, several stages of […] pumping in [series], you would get the vacuum that you needed on a continuous basis. You just…and you could run a conveyor belt in carefully graduated openings in order to reduce the airflow out, but you could get the kind of vacuum you needed for continuous sputtering.

There was work on new ways of electrodeposition…of metal deposition—I'm sorry, I should say—other than electrodeposition and sputtering processes, where you could photosensitize plastic film, and then use electroless plating, where you get plating just with a surface, and then activate, light activate [it].

Most of the staff there were from the Western Electric manufacturing engineering group. They'd pick people who were particularly inventive to come in and do that work. But in order to fulfill our charter, I thought we had to really hire PhDs in chemical engineering and chemistry […possibly] someone in <**T: 10 min**> physics who was really interested in applications, that sort of [person]. So, we started building a staff that way. Most of the laser work was done by new hires that we brought in after I got there.

It was just a heck of a lot of fun. One of my principal jobs was to establish more credibility for that lab with the manufacturing engineers who were going to be…who were our customers and who worked in less lovely environments than we did, and who had to solve everyday problems the moment that they happened, and who also were used to developing their own technology. So I would say I spent at least half my time out on the road visiting all the Western Electric locations, telling them what was going on, and convincing them we were just people like themselves who just had a different charter. We were there to help them, actually.

BROCK: Did that go fairly smoothly or was it rocky […]?

TANENBAUM: It went fairly smoothly in some areas, and pretty rocky in some other areas.

BROCK: Place by place, and person by person…

TANENBAUM: Place by place, person by person. That's exactly right. There was an engineer of manufacturing who was essentially the head engineer at each of our major plant locations. Western had big, big plants. Probably the average size plant was three or four thousand people. Some of the very big ones, the Hawthornes and the Kearnys were…well, during World War II, Hawthorne, I think was [well over forty] thousand [employees].

BROCK: That's enormous.

TANENBAUM: Yes. So these were big plants. They had big groups of competent engineers who were used to doing their own thing. Our job was to show them how we could help them do it better. But I liked that. That was a lot of fun.

BROCK: You liked that convincing role, that…?

TANENBAUM: Yes, absolutely.

BROCK: What about that appealed to you?

TANENBAUM: Just getting it done, seeing something happen, and doing whatever you needed to do: go out after work and drink beer or whatever it was to gain their confidence. Yes, you're not out there to beat them, you're out there to help them.

BROCK: What would you say you'd spend the other half of your time on?

TANENBAUM: Well, it was recruiting, building the staff, and getting…I had a lot to learn myself about manufacturing and about how plants really worked. I was able to learn a lot of that from some of the…well, just about all….every member of staff there, when I came in, knew more about manufacturing than I did.

BROCK: I'm sure the site visits also…

TANENBAUM: Oh, sure, absolutely. Yes.

BROCK: You were there for four years in that role. How large did the laboratory become, in terms of sort of professional staff, would you say?

TANENBAUM: I don't know what unit to use to answer that question. I suspect we were bringing in probably a dozen or so PhDs every year. The staff, when I got there, if I remember correctly, I think, was about three hundred total, and probably a little less than half of that were the professionals. So in the four years, and by the time I left, the staff had [grown] to about five hundred, if I recall correctly. The new hires: they were all either PhD or master degree people. So I suspect probably about a third of the professional staff, then, were people that were recruited during that period of time.

BROCK: So that must have taken quite a bit of your time to build that staff up.

TANENBAUM: Well, it did. I think the principal job at first was to convince the department heads, who reported directly to me, that this is what we needed to do <**T: 15 min**>. Again, that was not too difficult. That was not too difficult. The other job I had was to convince the Vice Presidents of Manufacturing who ran the plants that this money was being well spent. That…it was very interesting. I think there was a lot of pressure to close the place down when I first came there. Ray Cook, who was Tim Shae's successor, was fighting the battle.

They had not done nearly as good a job as they could in terms of pointing out the stuff that had already come out of the labs, also pointing out that the manufacturing challenge is that the plants were going to be facing as the solid-state electronics hit them…altogether a different kind of product. When I got there, I think it was…if it wasn't on my first day, it was during my first week, I was told that we have to prepare now for the visit of the Board of Directors of Western Electric. They were scheduled to come. I, of course, was supposed to explain to them everything that went on, and why we were there, and what it was costing, and what benefit it was to the company.

So my first couple of months were information gathering and making charts and slides and what have you, and throwing them away, and making another set. Fortunately, that [visit] got delayed a few months. So that was wonderful because I was ready for that first day, and then it got delayed and I had time to do something there. But that was a very important meeting, and it was followed by a meeting with all the senior officers of Western Electric. I think that went pretty well. They came away with a different view of what the Engineering Research Center was all about, and I think a better of understanding that it was all ready helping them and could help some more.

BROCK: Was part of the structure of the financing of the laboratory is that it was essentially got a subsidy from the different manufacturing…

TANENBAUM: No, it [was] corporate…

BROCK: …units?

TANENBAUM: It was corporate…

BROCK: Oh, okay.

TANENBAUM: …finance. So it was a hunk out of each vice president's budget. But the plants bore it…it was someplace in the general administration…[the opportunity to take on specific tasks from specific plants].

BROCK: I understand.

TANENBAUM: We did work toward…and it was getting started—I don't know how it ended up—but taking on specific tasks from specific plants where they then would fund it. We wanted to move in that direction. But the basic core support was from the corporate level.

BROCK: So almost like a contract research direction from the…

TANENBAUM: That's right.

BROCK: …different facilities.

TANENBAUM: That's right.

BROCK: And was…. A moment ago you talked about the big shift that these manufacturing facilities were going to be facing in the shift, I guess, essentially, what you might call like an electromechanical…

TANENBAUM: It was…

BROCK: …products to this solid-state electronics.

TANENBAUM: That's right. The great bulk of what Western Electric made was electromechanical. The basic switches were all electromechanical. Electronic switching had started, but it was just learning its way, and [solid-state] was all discrete devices, so it still had trouble competing with the electromechanical. You could offer better services, you could do better diagnostics and what have you, but that's a little harder to sell to the telephone companies—until the cost…until the switches became really cost competitive, as well as offering the other services. That's why Western Electric made relays and [electromechanical] crossbar switches by the thousands, hundreds of thousands.

BROCK: But in this period when, sort of, they're given renewed vigor, let's say, to this manufacturing research effort and laboratory that you're directing, do you think that was part of <**T: 20 min**> the reason why they wanted to put more emphasis there and get it applying basic science to manufacturing itself, was because, as an organization, Western Electric could see that they were going to have this phase change, really change the nature of what kind of a manufacturing organization they were?

TANENBAUM: I don't think you could say that Western Electric did that. I think you could say that Tim Shae did that.

BROCK: Okay.

TANENBAUM: And it was his idea and Ray Cook followed and supported it. But Tim was a man of great influence in Western Electric, and Ray was more of a newcomer, at least in that area—he did not have Tim's credibility. So I really attribute that foresightedness to one individual, who had high credibility within the organization, and who pulled it off.

One of the things I did after, I guess, it was my first year, after I felt had some reasonable idea of what the job was, was to ask myself the question, "What are other people doing in this area?" I couldn't find much, either here in the U.S…we went to Europe and visited a number of our licensees there, and we raised the question. They really hadn't thought about it as far as you could tell. In Japan as well. So I think Western Electric was really a leader in that area. [There] was very little we could learn [from others]. The only that thing came close to it was in the Netherlands: the government had supported some work looking at truly advanced manufacturing technology.

BROCK: But even places like…there were no cognate organizations in something like an IBM [International Business Machines Corporation] or…?

TANENBAUM: Well, IBM at that time was already all electronic. But they did not have…if you asked them, "What are you doing in manufacturing research?," about the only thing they would think about was the industrial engineering kind of things: product flow and using computers. IBM was very active [in computer applications], of course. They wanted to get more computers on the manufacturing floor. So in that area, they were at least as far along, I think, probably further along than we were at ERC.

But if you get over in the question of how you…and IBM was pretty much solid-state committed by that time. So they had already made the step. It was still ahead of Western Electric. But they did not have what I would consider, what anyone could call, an ongoing effort to develop that, [say], of just introducing new manufacturing technology. The only places that started to come close to that were the people who made the tools that are used in manufacturing. [One was] Cincinnati Milling Machines, you know the people who made the computer operated, early computer operated milling machines […] and what have you, and material handling kinds of things. But you couldn't find any lasers in those places.

BROCK: Well, two questions come to mind. One is, when do you think that Western Electric did make the sort of solid-state switch? When was that in time?

TANENBAUM: Well, it was a continuous process. I don't think you would…the first [big] step was, number one, ESS [Electronic Switching System], and then it started finding its way into telephones. The tone generator, I think, was probably one of the very first applications. But it was a continuous process as the cost of semiconductors came down and, of course, the introduction of integrated circuitry <**T: 25 min**> was the major step there. Then the applications started to blossom.

BROCK: Well, maybe a better way to ask the question would be, "When do you think that was complete, that transition?"

TANENBAUM: Well, I think…you know I'd have to go back and look. I would identify that with the last crossbar switch that was shipped out of Western Electric. I would guess that was twenty years ago.

BROCK: Right. Because there had to be also a transition of continuing to manufacture…

TANENBAUM: Oh sure…

BROCK: …replacement parts until you're ready to…

TANENBAUM: That's right.

BROCK: …overhaul that section of the system.

TANENBAUM: Right. It's more than just replacement parts, because telephone switches have to grow. As areas develop, you put a small switch in, and then each of the switches has to be designed so it can double, triple, quadruple in size. And once you started electromechanically, you continue until that office has reached its size or reached a point where it is economically feasible to replace it.

BROCK: Right.

TANENBAUM: So I don't…there are undoubtedly still crossbar switches out there, and…but I don't think very many of them are growing. The last manual switchboard went out, I think, only about fifteen years ago.

BROCK: Really.

TANENBAUM: A little place in Maine, I think it was.

BROCK: Well, in looking at that four-year period, ‘64 to ‘68, when you’re running the ERC, what would you say your major successes were and your major failures, if there were any?

TANENBAUM: Well, the major success, I think, was getting acceptance of the ERC as an integral part of the Western Electric community. There were a number of things that I…well, it was a continuous: we established processes [for] bringing [in] promising new engineers that were hired in Western Electric plants and had been there two years—they’d come in as interns for a year or so; and we’d arrange for some of our people to go out to the plants; and just a whole bunch of things [like] that.

Important process developments: one of them that comes immediately to mind has to do with the extrusion of plastics. Western [Electric] did an awful lot of that in, for example, coatings on cables, as well as telephone parts and what have you. The design of the extrusion screw was an art. […] You had certain people [who] had a lot of experience and they would design the screws for different kinds of applications.

We hired a young man, an Israeli […] who…a PhD chemical engineer, [Zehev Tadmor],who had done his thesis work in that area, who came in and looked at the experimental work and also developed the algorithms to quantitatively design an extrusion screw which became used throughout Western Electric. He later went back to Israel and he was the president of the Technion[-Israeli Institute of Technology] for a number of years. [He has] since [retired], I think.

BROCK: When you were a year into the job, and you were casting around to see if there were other models out there for such an effort, and you didn’t find any, over time did you find other industrial corporations, manufacturing corporations, coming to your laboratory to visit and see it as a model?

TANENBAUM: Not particularly. No, I think that the…not while I was there, at any rate. I really don’t know what happened afterwards, but I was there for <**T: 30 min**>, what was it? Four years, I guess.

BROCK: Yes.

TANENBAUM: Yes. And I really don't know what's going on in that area today.

BROCK: And neither do I. But that laboratory, that continued for quite a while in the Western Electric System?

TANENBAUM: Yes, it did. It did. It…I know it was still active after the Lucent [Technologies] spinoff. It went with Lucent. I don't know whether…I don't even know whether it's still in existence or not today. And Lucent is gone, too. So much turmoil, I would be a bit surprised if it doesn't exist.

BROCK: But it did have a…

TANENBAUM: It became…

BROCK: …until very recently…

TANENBAUM: Yes. It actually became…

[END OF AUDIO, FILE 2.2]

BROCK: Okay.

TANENBAUM: Now there [was] major restructuring of Western Electric after it was spun off. I don't recall whether ERC became a part of Bell Labs during that period or not.

BROCK: What do you think were the major lessons that you personally learned in managing technological innovation, for lack of a more precise term, of managing these sorts of efforts?

TANENBAUM: What are the guiding principles?

BROCK: Yes.

TANENBAUM: I think it's basically a human process, and a question of building bridges, communications bridges, understanding bridges, credibility of bridges between people with different kinds of motivations. In Bell Labs, for example—and I think you find it endemic in many parts of universities—there was long, and I suspect still exists (I don't know to what degree), a barrier between the people doing fundamental science who just aren't interested and actually look down upon, people doing engineering and applications. And finding ways…and similarly, people who are doing that with inventions, new inventions, which are often competitive with well-established technologies, or well-established products, have a barrier there, too, of demonstrating what they have is superior and economically attractive versus what has all ready been out there for quite some time.

One of the important aspects of Bell Laboratories was the fact that, at least when I was here, which was some time ago, the understanding that the science that was done here, and you really wanted to be on the leading edge, the fundamental edge, but it was there for a purpose other than just expanding knowledge. It was there for the purpose of solving problems in telecommunications and opening new opportunities in telecommunications. As I think I said when we first started this, when I first came here I was told, "You can do anything you want as long as we believe it has promise [for the business]…

BROCK: Right.

TANENBAUM: …any discoveries of any understanding [that] has promise in the application." One of the principal jobs of management in research was to have enough understanding of what the problems and opportunities in telecommunications are, so that you could help identify whether—new discoveries—whether they were done here in Bell Labs or someplace else. The fellows that had been working well, they will know what's going on in someplace else in basic research. So it is really building those bridges and overcoming rather natural human tendencies. That, to my mind, is what innovation is all about and what managing innovation is all about.

BROCK: Well, so when we get to 1968, then, I guess a new opportunity for you arises or new offer, or yet again, somebody comes to speak to you about making a shift. This was to be general manager of Western Electric's engineering division, if I'm correct. Can you tell me about how <**T: 05 min**> […the] story of that move?

TANENBAUM: Sure. That to me was, just my personal point of view, the best measure of my own success in doing my job at ERC that I could have thought of, because it meant to me that I had gained credibility with all the heads of manufacturing engineering and all the plants, as well as their plant managers and vice presidents. So I felt very good about that. Ray Cook was my boss…would be my boss. But my job was really to…the principal…let me say it this way: the principal power of that position was that division decided which products would go to which plants for manufacturing. That was a tremendous leverage in terms of approaching the various plants and their manufacturing organizations, and looking at them, and helping them decide what changes maybe ought to be made, and how good their personnel is, and whether upgrading the personnel is a necessary and desirable thing. As well as, also, a good position in terms of continuing to build bridges between ERC and the manufacturing plants.

So I probably did more traveling in that job than I had ever done in any other job that I've ever had. We had annual conferences with all the leaders of engineering. I was responsible for those conferences, as well as played an active role in deciding who would be…when we needed a replacement [for the chief engineer at a plant and] who would be the natural replacement. It was an interesting sort of thing, because the direct line between the director of manufacturing engineering in a plant was to the plant manager, whose direct line was to the vice president. I was on the same level as the plant manager, essentially.

BROCK: Okay.

TANENBAUM: So I had what was kind of a dotted-line responsibility, but it was a very, very small dotted-line relationship. So that's essentially what that job was: to get that done.

BROCK: So it involved, really, a broad evaluation of each facility. That, if I'm understanding you correctly, that, sort of, the assignment of what gets made where, that, in and of itself, I'm sure, is a very important decision, but it also allowed you to facilitate other changes that people wanted to see happen…

TANENBAUM: Well…

BROCK: …by using that, or…?

TANENBAUM: Well, yes. Often, that's a very straightforward matter, because a plant is making a certain product. Along come some new elements that have to be made to become part of that product expanding, so that's fairly straightforward. Real decisions come when you decide that you want to source a product to two plants, and who's the new plant, or when an altogether new product comes out, who gets it. That new product is likely to displace an existing product. So that task has a great deal of leverage.

Now, at the same time you have to be wise about that, because if you do something that appears to be…doesn't make much sense, but you're doing it because you want to [leverage] something, that ain't gonna work. Because that goes immediately right up the ladder and you have to defend in a reasonable way what it is you wanted to do. But it…the <**T: 10 min**> hierarchical structure in Western Electric—and I think it's probably true in most manufacturing organizations—is very strong. The discipline is very strong. It's not like running a research group. I mean, once a decision is made, everybody marches. But getting the decision made is the critical part.

Western Electric was a very efficient, very efficient manufacturing organization. It was investigated by the FCC [Federal Communications Commission] who got Touche, Ross, [and Company], I guess it was, to do an overall study of Western Electric, because the questions were raised [from competitors about] how does…"Western makes all this stuff, and we make it better and cheaper." The Touche Ross report was very, very supportive [of the work at] Western, [operations and efficiency]. So you weren't dealing with problem areas, I mean, obvious problem areas. What you were trying to do was to improve on something that was already pretty good [and] also prepare it for what you could see as major changes coming down the road, so it will continue to be very good.

BROCK: Right. You just mentioned the hierarchical, sort of, structure of the big manufacturing concern. Did that…did you experience culture shock coming from Bell Labs and then, more and more, getting involved in…I mean, essentially going to the ERC, I would imagine that's sort of the laboratory…

TANENBAUM: Yes.

BROCK: …but then in this new role, was that when…well, did you experience culture shock of a sort?

TANENBAUM: Well, I don't think there was…I wouldn't call it a shock. A shock to me means surprise. I'd had enough contact in my last jobs at Bell Labs both, direct contact as well as hearing the miseries and complaints of my colleagues at Bell Labs whose job it was to work with Western, to gain some understanding of what was going on. So it wasn't a shock. It was certainly quite a difference in culture.

BROCK: Did you find it…did you like it or did you…?

TANENBAUM: Oh, I really liked it. Well, I expected it. If I…that's why I say it wasn't a shock. No. I could see it had some strong points, but it also had some things it had to change.

BROCK: What had to change?

TANENBAUM: Well, one thing that had to change was that you needed more educated engineers than you had; Western was used to growing its own engineers. You would find in that engineering force some really very, very capable people who never went to college at all. But on the average you knew, first of all, those very, very good people, today, would have gone to college, and that's where you were going to get them. Unless you were going to need people with…who could understand and communicate a lot better, like in the electronic arena as contrasted to electromechanical arena.

BROCK: Right. And in that role as general manager, were you working out of the corporate headquarters? Or…what was that like?

TANENBAUM: Actually, the corporate headquarters of Western Electric was located right across the street from the AT&T corporate headquarters, Fulton [Street] and Broadway [Avenue in New York City], and they were catty-cornered. But the top officers of Western Electric [had offices] in the AT&T building, a floor in the AT&T building. That's where my office was. I had a beautiful view of Manhattan, [New York], City Hall….

BROCK: Then did you move back? On a personal note, did you move out of Hopewell?

TANENBAUM: Oh, no, no, no. We stayed. We lived in Rocky Hill, [New Jersey], actually…

BROCK: Okay.

TANENBAUM: …a suburb, a little suburb of Princeton. No. That was my first commute to…and I hated it. People <**T: 15 min**> told me I'd get used to it, but I hated it more and more every day.

BROCK: Was this move to this general manager position…did you feel that you were embarking on a known different career path into more of an…

TANENBAUM: Well, I…

BROCK: …executive officer role?

TANENBAUM: Oh, I see from that point of view.

BROCK: Yes.

TANENBAUM: Yes. The big transition in my life was becoming a first-level manager of research at Bell Labs. That was the big change. From that point on, it was sort of gradual, a gradual evolution. Up to that point I was…everything I was in, although I was moving up the ranks of management, was still a very strong, technically-oriented [position]. But I was getting interested in the business aspects, because I could see, particularly in the…when I became general manager of engineering, I had oversight of the whole cost-reduction process. Western Electric had a very formalized cost-reduction process. The engineering groups were measured in terms of what they produced and cost reduction. The cost reduction was audited and accounted for very, very explicitly. You wrote a case that said, “This is what we’re going to do; this is what it’s going to cost to do it; this is how much it’s going to save per part,” for example. Particularly, in the…you’re not talking about new products now. You’re talking…“This is how much it’s going to save.” There was a standard costing procedure in Western Electric, so that you knew whether it saved it or not when it was introduced. You could tell; the numbers showed. So managing that, you start to learn a lot about the financial aspects of a business. And I found that very interesting.

BROCK: Just for its quantitative nature or…?

TANENBAUM: Well both for its quantitative nature and also for an understanding of what actually drives our economy. We’re what…[when] people talked about the bottom line, okay, this is what it is.

BROCK: So was this, being in the AT&T building, headquarters, was that again a shift for you in the atmosphere? What was it…what was that like?

TANENBAUM: Well, it was like being in Western Electric. We were located in the building. I had started to have more contact with AT&T in my next job, rather than in this one. This one was still pretty much all Western Electric-oriented.

BROCK: Well, maybe we could talk about that. Then, it was in 1971 that you were promoted I guess that was to Vice President for the Engineering Division of Western Electric. So what was that change like? What new role did you play?

TANENBAUM: Well, the principal new role was a direct responsibility for the Bell Labs' budget that Western Electric funded. Western Electric funded about 75 to 80 percent of Bell Labs.

BROCK: Wow.

TANENBAUM: Everything that…once something became a project or a product, once a decision was made that we're going to design a product, Western Electric started the funding. Basic research at Bell Labs was a relatively small part of the whole operation. So that was the principal new responsibility.

Along with that was…I guess, that was probably the other…what I was going to mention was probably the responsibility of the <**T: 20 min**> prior job, which was making and approving the cost estimates for new products and new systems. These estimates would be…well, Bell Labs would make the estimates at the beginning and then as products started to evolve, Western Electric was making the estimates. So, you had to make the estimates and, although you were not responsible for pricing, your estimates were the basis for pricing. But that was…and finally that had to be approved by the vice president for any major project of any significance. But most of that work was done by the general manager of engineering.

But the really big thing there was, at the vice presidential level of course, was oversight of everything the general manager of engineering did, but also direct oversight of the Western Electric budget at Bell Labs. So the principal thing that I did there with a moderate amount of smoke and fire in Bell Labs was to introduce a case approach to…financial case approach to the Western Electric [development] budget at Bell Labs—namely, a requirement that before we would approve a major new project, we would require an economic analysis of it: what the sales were going to be; and what the costs were going to be; what the profitability was going to be and discounted cash flow; evaluation of when it would break even in terms of cost; and whether the project should go ahead or not.

Bell Labs was used to doing whatever was done there itself. It was never a formalized mechanism. They would have their estimates. For example, electronic switching: big project, hundreds of millions of dollars to be spent over a number of years.

BROCK: Is this an example that you worked on in the period or…?

TANENBAUM: No. No. One of the examples I worked on in the period was picture phone. A very dubious project from a financial point of view, primarily because you couldn't…no one, no one could agree as to what the market's going to be.

BROCK: I'm sorry if I interrupted what you were…

TANENBAUM: No, that's all right.

BROCK: …saying for the electronic switching.

TANENBAUM: Well, all I was saying there was that if you went and said, "Well, where is the case analysis for this project?," you wouldn't find it. It was based on Bell Laboratories' estimates of what it would cost to manufacture it. There may have been some Western input in that, but it was never formalized.

BROCK: And when you went to this mode, I would imagine that the people at Bell Labs would need some assistance with that, you know, to make a solid…

TANENBAUM: Well, you'd never get them to agree to that. The first thing you had to do was get them to agree, and you couldn't get…nobody up here said, "Gee, that's a good idea." They thought work was unnecessary, and they knew this [project] was going to be a much better system for the Bell System, and, "What do you want all this paperwork for?" Well, I had learned, starting when I had to write my first technical paper, that although you think you understand everything you've done, when you start to put it down in writing, you start to find out that there's a lot more work you've got to do before you can publish. The same thing applies, probably in spades, to a financial case on the introduction of a new product.

So, finally, the first thing we did was say, "Let's take a particular project that's coming along and try to <**T: 25 min**> see what we can do." We got Bell Labs to agree to it by my simply saying, "Until we have it, it's not going to be approved." That, of course, went up to my boss, and my boss' boss. But it took hold. Now, I must say, I don't really know what happened to it. But that was, if I had to pick out one thing that I tried to get done and did get started during the relatively short period I was in that job, that was it.

BROCK: In this period of time, the early…well, '71, '72, most of the products that Western Electric would make…most of the products would be…basically the customer was AT&T for them, was that right?

TANENBAUM: The Bell Operating Companies…

BROCK: Yes, okay.

TANENBAUM: Oh, yes. We were…

BROCK: Very little […]

TANENBAUM: We were back in consent decree. We were not…we could not make any product that was not for telecommunications. Telecommunications was the Bell System in the U.S.

BROCK: But for things like a picture phone or, you know, some sort of new handset, I guess there would be…then thinking about the market, you know, you're thinking about sort of that end user who is going to use it in a handset or the picture phone.

TANENBAUM: Yes. Yes, that's right.

BROCK: Many of these others are…

TANENBAUM: That's right. Of course the Bell Operating Companies were the ones closest to the customer.

BROCK: Right.

TANENBAUM: They would… they were the ones who finally decided whether to buy the stuff or not.

BROCK: From a switch to…

TANENBAUM: To a telephone, absolutely, sure. You've got to remember that during most of that period of time, telephones were owned by the telephone company.

BROCK: Right. So it's interesting, because there's a very…well, it's an interesting sort of situation, it would seem to me, from a producer's manufacturing side, is that your main customers are so inter-linked, and the end consumer is really kind of far away.

TANENBAUM: Yes. But that job, that market—call it the marketing job—that was done by AT&T, AT&T working with the Operating Companies. I mean, Bell Labs would have its estimates and Bell Labs would go off on its own sometime and do interviews and what have you. That was always frowned upon, but it got done anyway, I am told.

But fundamentally, it was AT&T. AT&T made the decision—that was later my job at AT&T: made the decision of what equipment would be used by the telephone companies. But that decision was based upon a lot of strong interaction with telephone companies, and surveys. We'd get independent surveys made, market surveys. But in the final analysis, that had to be the input. When this financial case that we wanted was supposed to have in it AT&T's numbers with regard to marketing.

BROCK: Okay. Well, you were in that Vice President for the Engineering Division post, very briefly. In 1972, I believe, you became Vice President for Manufacturing of Transmission Equipment. Now, I was wondering, what sort of a movement was that? Was the Engineering Division vice president then reporting to you or was it…

TANENBAUM: Well…

BROCK: I couldn't figure that out.

TANENBAUM: Yes. It was, from <**T: 30 min**> an administrative chart point of view, it was a parallel position. I was on vacation, actually, with my family on Prince Edward Island and got a call from…it was probably Joe West, I guess—Joe West who was the executive vice president for manufacturing at Western Electric—telling me that the Vice President of Manufacturing Transmission Equipment has died of a heart attack unexpectedly. He was a man in his fifties, I guess. They wanted me to take that job. I guess they assumed it was about time I got out there…

[END OF AUDIO, FILE 2.3]

BROCK: Okay, great.

TANENBAUM: This was going to require a geographical move because the headquarters of that division was in…were in Boston. So, [my family and I] talked about it and decided that was what I ought to do. So, we cut our vacation short, got in the car, and drove back here. That job…hard to think of how many…one, two, three, four, about five plants in that division and it made all the transmission equipment from…didn't make wire and cable, but it made all the electrical and electronic equipment that went with it from a two-line carrier [that] you use in rural areas to—we had long runs—to the high-speed microwave. At the time, we were looking at both millimeter wave guide and optical fiber. So it encompassed all of the electronics [and photonics that were] associated with that.

BROCK: Was this period of the early '70s…was this a particularly important time in the development of transmission technology? And what was being deployed actually out and about?

TANENBAUM: Yes. It was. It was just the beginning of the optical fiber stuff. But it was also a transition from one other point of view, that we were just starting to see really serious competition for equipment, for suppliers, outside the Bell system.

BROCK: Could you say a little bit more about that?

TANENBAUM: Sure. The equipment that was coming under [competition] was a digital carrier, the first digital carrier, digital transmission equipment, which AT&T had introduced several years before that, called them T-1 carrier.

BROCK: Okay.

TANENBAUM: You've heard of that?

BROCK: Yes.

TANENBAUM: The competition was IT&T [International Telephone and Telegraph Corporation]—the principal competition at that time. There had been competition in microwave by Collins Company, but they were always a fairly small company. They sold equipment primarily to the independent telephone companies rather than to AT&T. But there was always pressure on AT&T from the outside, from the FCC [and the U.S. Department of Justice], to buy other equipment, other than Western Electric. We were constantly required to document why Western Electric equipment was chosen on the basis of price, […] performance, reliability, and so forth. The telephone companies…for the most part that was an interaction between AT&T and the FCC. AT&T was where that interface was.

But we also had, I would call them "constructively aggressive" people out in the telephone companies, who wanted to try somebody else's equipment. Some of them…and that came mostly, I would guess, from one telephone company in particular, Southern New England Telephone [Company], which was only 25 percent owned by AT&T. That was one of the few telephone companies where, it was part of the Bell system…there were two of them, where a majority owned outside of Bell systems. That was one <**T: 05 min**>, and Cincinnati Telephone [Cincinnati Bell] was the other one.

The chief engineer [John Craig] of Southern New England—oh, golly, I knew I was going to forget his name—but, his father [Cleo Craig] had been the president of AT&T at one time. He was an interesting guy. He always liked to pull AT&T's tail whenever he could. But they would often buy other equipment and put it in their plant and see how it worked. For the most part, though, they were still strong Western Electric customers. But they got very interested in the product IT&T was making, a digital T-1 carrier product, and then started installing a good deal of it. We had looked at it. It was an okay product. There were parts of it that we worried about: the plug-in cards were not protected, physically protected. Because of that they were able to make it cheaper. It's amazing how much of the cost of some of that equipment is simply in the steel frames that you mount it as contrasted to the electronics.

It was at that time that we decided that…Western Electric decided that, "We've really got to get much more serious about marketing and understanding all these other products, and building, strengthening, our relationships with the…become a marketing-oriented company more than"…it used to be just simply relied on AT&T to tell the operating company, "You buy that." But it became clear, and I think, AT&T was starting to understand also, that competitive pressures were becoming more and more important.

So because this was a [transmission] product, my division became the site, essentially, for looking at how to become a marketing company. AT&T got McKinsey [& Company, Inc.] involved and we lived with McKinsey people for quite a while—good people, smart people—looking at this particular product and what's attractive about the competition. There were a few other smaller manufacturers, but IT&T was the big one. So that…I wasn't on that job very long, […] but that was probably the major thing. The one other…the other thing was that we built one of our first small plants. We decided that our large plants weren't really flexible enough. So we built…the first one was a transmission plant in California. We also had a lot of pressure from California from the PUC [Public Utilities Commission] out there to get some manufacturing activity. This was in San Ramon, California, which is right off the [San Francisco] Bay, south of San Francisco, but on the eastern side of the Bay…the western side. We also started a plant in Richmond, [Virginia], for making printed circuits.

So we had two plants start up at the same time. So, it was a busy time, but a fun time. I had a wonderful office and down…one of the first…it was [in] the second skyscraper in Boston. The first one was the Prudential Building. This was One State Street. I had an office that overlooked the airport. You saw the planes come in, just saw the whole thing, just a wonderful office. Well, I didn't spend much time in it, but it was a wonderful office.

BROCK: Why was this group located in Boston?

TANENBAUM: The principal transmission equipment plant was in Andover, Massachusetts. It was that plant's general manager [Harry Snook] who became the first Vice President for Transmission Equipment. All the plants prior to had reported directly in—I should have mentioned this earlier—had reported directly to the Executive Vice President for Manufacturing at Western Electric corporate headquarters. But, I don't know, five years or so prior to this time <**T: 10 min**>, that was getting unwieldy. So they created product divisions with a vice president. The general manager of the Merrimack Valley plant, which was the largest transmission plant, became the Vice President for Transmission Equipment. He located his office in Boston.

BROCK: Was the whole issue of…were satellite communications part of the transmission area?

TANENBAUM: Well, we were kicked out of that business by the federal government…

BROCK: Oh, really.

TANENBAUM: You know we…after we launched Telstar, which we did on our own, essentially. We had to have government permission to get the launch vehicle blasted off. They decided they didn't want AT&T in that business. It was too big of a monopoly as it was, so, "Give it some competition." So that's when the…what was it, Comsat [Communications Satellite Corporation] was formed. We were simply told, "You can't be in that business. Comsat's going to do it."

BROCK: I did not realize that.

TANENBAUM: As I mentioned earlier, my group made some of the parts that went into Telstar, but that was the last communication satellite AT&T was permitted to send up […].

BROCK: [recorder off]

We were talking about VP of Manufacturing for Transmission Equipment. I was wondering your thoughts about being a scientist or coming from a technical formation and serving in that role, what that may have added to what you were able to do there or was that something that…the previous vice president had been a technical person? Or, you know how did that work?

TANENBAUM: To answer your last question first, I'm not sure. I really don't know. I don't know whether Harry Snook came from an engineering background or not. And I'm not really sure how to answer the other question. It was certainly helpful, I think. At least I found it helpful to understand technically what the stuff we were making does and how it does it. Of course everything was dominated by silicon technology at that time. I had, of course, never had any electrical engineering training myself, but I learned it as I went along, as I had to.

So it made me…I felt comfortable talking to Bell Labs people and talking about new products, [and], because of my immediately previous experience, understanding something about the economics and financial analysis, and what have you. I think probably the most important thing was that I was accustomed to trying to dig down to understand things, to ask questions and understand things and demand a kind of logical, if you will—at least what I considered to be logical—explanation to those questions.

I think you get the same sort of thing that you do when you were doing science. Is it the science…it's related to the scientific method. I think that kind of thinking underlies it. So I suspect all that helped me. If I hadn't had my scientific background, I think I probably just normally tended to look for logical answers to things. That's what drew me to science, probably. But my technical background was…certainly gave me a good deal of comfort in the job.

BROCK: Were other of your Western Electric colleagues at this vice president level…did they all have technical backgrounds? Or did some of them come from quite different backgrounds?

TANENBAUM: Well, a number of them did, but not all of them. But I <**T: 15 min**> think that was a reasonably natural path for Western Electric.

BROCK: Well, […] you were in this post a little bit longer than the one before, until 1975, if I'm right, at which time you returned, then, to Bell Laboratories as Executive Vice President for Systems Engineering and Development. Could you tell me the story of the return to Bell Labs?

TANENBAUM: I had kind of mixed feelings about it, to tell you the truth. I mean, I considered it a great compliment to be asked to do that. At the same time, I was getting more and more interested in the business aspects. The job I got certainly required some business interest, but it seemed to me to be a bit of a deviation from the path I thought I was on. It was becoming clear to me, then, that I was being aimed to become President of the Bell Laboratories. While that would have…that's a wonderful post and a wonderful job, it just wasn't where I wanted to end my career.

BROCK: Because of your real engagement with the business aspects?

TANENBAUM: Right.

BROCK: And who do you think was aiming you towards the presidency of Bell Labs?

TANENBAUM: Well, I had to…I don't know how to answer that question. I think the Western Electric people would have been very supportive of that because they had gotten to know me. I think…I'm trying to think whether Jim Fisk was still President of Bell Labs, or whether it was Bill Baker at that time. I don't remember exactly when Jim Fisk retired. But I knew them both, and they both knew me. So was there a particular individual, I don't know that. I don't know that. It wasn't really…it was later on, when I was in AT&T that someone told me that that's what they [had] expected. I told them at the time that that would not be my first choice.

BROCK: Well, how did the offer of this post come about, and what was it?

TANENBAUM: I think it came about…there was some trouble in Bell Labs at that time. It was Bill Baker who was the…Bill Baker was the President of Bell Labs at that time. They'd had a major, major problem in the semiconductor area, where they were using aluminum interconnections […] rather than gold—you know, cost reduction. But they were failing. In spite of all the testing that they had done on them, they were failing. That was big. It was a big issue. Then, there was one of the vice presidents who was not…the vice president of that division [of Bell Labs], actually, was not well. So there were those problems. I suspect that Western wanted someone there with…who they felt understood what Western needed.

BROCK: Right. Was it the problem that the semiconductor devices over in manufacturing were having these problems with the metallization?

TANENBAUM: Well, no, out of field failures after the product was shipped.

BROCK: Oh, wow.

TANENBAUM: Yes. When <**T: 20 min**> the old Bell system made a switch, the numbers were always large, so very few minor problems.

BROCK: These semiconductor devices were being used as what in the…?

TANENBAUM: Oh, just everywhere.

BROCK: Oh, just all over…

TANENBAUM: Yes. Yes, everywhere. Yes.

BROCK: So a huge problem.

TANENBAUM: So a real problem. But Western wasn't happy about how it was being handled. They didn't think it was getting the attention that it really should have. But…so I'm just trying to think whether someone retired at that time or…I don't think that was the time when Molnar died. It was…Molnar, I mentioned earlier. I'm inclined to think that was a position that was created, but I'm just not sure of that. I just don't remember that; didn't make much difference to me.

So we moved back to New Jersey, and I came in on that job. I was on that job, I think, for eleven months, if I recall. But there was one very important thing that happened during that period of time. I have to go back a bit. Western Electric, Bell Labs/Western Electric…the Bell System introduced [electronic] switching for the first time. I don't remember the exact date, but it was several years before that. But it was introduced for [local switches]. In the network you start off with local switches, which connect then for calls outside the local area to so-called "toll" [switching systems]. Now, there's a somewhat more complicated hierarchy than that, but that's fundamentally it. There was a lot of digital carrier in use at that time to connect between the local offices to the high-volume toll offices. So it became attractive to start seriously considering digital switching rather than analog switching there, where you would switch digits rather than actually switch connections. That machine…that was a number 4 ESS. That was a very successful and big project. Far and away the biggest project that Bell Labs had ever undertaken, and the first switch was opened, I think, in Chicago. I have the date at home [17 January 1976], but I don't remember the date.

At any rate, a question kept coming up, "Well, should we move to digital switching at the local offices?" Well, number 1 ESS was doing very well, and its production had reached a high level, but it was an analog switch. Instead of switching digits, it comes in—[as the] Internet switches packages [of digits] now—it had metallic cross-connections, […] reed switches, actually—two conductors and a glass envelope and activated by a solenoid around it. It was also during this…a little before that time that the Northern Telecom, which used to be called Northern Electric, and was a subsidiary of Bell Canada, and was actually started by Western Electric…. There were always close cross-licensing arrangements there. But Northern…I think it was still called Northern Electric back then. I don't know when it became Nortel, but they're Nortel now.

BROCK: Oh, okay.

TANENBAUM: It was Northern Telecom and [then] Nortel […]. Led by a Bell Labs guy, who went up there, a Canadian, either Canadian or Brit, Don [Donald A.] Chisholm. I'm not sure: he's one or the other. At any rate, he was recruited by them in order to beef up their R&D activity. They developed a local switch, a digital local switch, the first digital local switch. We did the first digital switch, but it was for the big <**T: 25 min**> volume toll switch.

BROCK: Right.

TANENBAUM: Those turned out to be…started to be very interesting switches. Meanwhile, integrated circuitry had come in, and it was making digital switching look very much more attractive. I don't think you can ever have justified a local [digital] switch built with individual transistors, discrete transistors. But the integrated circuitry made that attractive. There were a number of the local telephone companies, Bell companies, that said, "When is AT&T going to have a digital local switch?" In fact, a few of them started buying one or two [Nortel] switches. Western Electric had been pushing AT&T to agree to standardize a digital switch so [that] Western Electric could get on with the development.

In fact, although this wasn't my business (I was in the transmission side), I was usually an auditor at the discussions that went on about that. When I came here, I thought that had been agreed to, that Bell Labs would start development of a digital switch. Well, I hadn't been here for more than a month or so, when I got a call from John [S.] Mayo…and John later became President of Bell Labs. At that time, he was an executive director in the switching area. He came to me, and he said, "Morrie. […] You know, there's been a lot of talk about [local] digital switching. We've been watching it, and it's really coming in, and it's really going to be a winner." He said, "We need to get on with it." And I said, "John, I thought we *were* on with it." He said, "Oh, no. […] Bell Labs is…you know, we've had some exploratory work, but there's been no agreement that we would develop the switch." I said, "Well, starting right now, go. […] I'm going to have to talk to the Western Electric people, but I don't think there's any problem about that at all. So you go."

I called Joe West, who was the Executive Vice President of Western Electric. He was startled, too, to learn that Bell Labs had not gotten final approval. So they did get on with it, and Western made a very successful…most switching now is digital. But in my eleven months that would be the standout thing, though we did solve…we did have to remove the vice president of…in charge of semiconductors. Actually, got John Mayo into that job and that problem got the attention it needed.

BROCK: Was there a big technical challenge in making a digital local switch as opposed to this high-volume toll lines…?

TANENBAUM: Well the…the real problem there was it was really an economic problem solved by technology. But from home or business back then, and still from home, but not business, it's analog. So you get to the switch, and although much of what's going out is digital, you've got to make that digital-to-analog conversion on every line, line-by-line. On the [outgoing side], you already multiplexed a lot of things together so you got much higher volumes. You can digitize that. But the real problem is being able to pay for the electronics that does the analog-to-digital conversion on the customer side of the switch. So that was a real challenge and that was a question of the electronics getting to the point, you know, integrated circuitry bringing the electronics cost down to the point where that was economically viable.

BROCK: I see.

TANENBAUM: Most of the other problems—the common control, the computer control, what have you—had been solved to some degree in the first […] analog switch, [1 ESS], but then, particularly in the digital, the big toll switches…so the toll switches: it's digital coming in, digital going out, so it's not an issue.

BROCK: So after…I guess moving back <**T: 30 min**> to New Jersey from Boston and only eleven months back at the Labs, you take up the role of Vice President for Engineering and Network Services at AT&T. So a new company…

TANENBAUM: Yes.

BROCK: What was the story of that quick transition?

TANENBAUM: Well the vice president…actually, again, I think that was associated with a restructuring at AT&T. AT&T used to have a job called the Chief Engineer, who had responsibility for all…

[END OF AUDIO, FILE 2.4]

TANENBAUM: [Before] AT&T restructured, […] there was an operating vice president who had responsibility for [all] operations. AT&T restructured into two vice presidents: one responsible for network services operations and all of engineering, and the other one for customer services and operations. So the network services, essentially, was associated with long distance, long [lines]—not totally, but mostly. Customer services was associated with telephones and PBXs [Private Branch Exchanges] and outside plant, everything that was…no, no. I'm sorry. Outside plant was part of network services. But it was really associated with the principal place of the business where competition was entering, because at that stage of the game, [people] could buy their own PBXs. They didn't have to use Bell system PBXs. They could buy and own their own telephones. So the customer services job was a major marketing job, as well as a technical job.

BROCK: And those two…

TANENBAUM: I think…I don't know whether I was the first Vice President of Network Services and Systems Engineering or the second. I don't know. I don't remember.

BROCK: Who did those two vice presidents, in turn, report to?

TANENBAUM: [An Executive Vice President who reported to] the President.

BROCK: The president.

TANENBAUM: Not the CEO [Chief Executive Officer], but the President.

BROCK: Okay. Then the President reported to the…?

TANENBAUM: The CEO.

BROCK: To the CEO.

TANENBAUM: Yes.

BROCK: Were those people…the [CEO] of AT&T, was he someone that you had known…?

TANENBAUM: Yes.

BROCK: Okay. Who was that?

TANENBAUM: At the time I came in, it was Hi [H.I.] Romnes, who had, actually, I think, been the only CEO who had started at Bell Labs. He didn't much time there.

BROCK: Oh, yes.

TANENBAUM: Yes, in the development area.

BROCK: Was it he…

TANENBAUM: Wait, wait…

BROCK: …who…?

TANENBAUM: …let me see. Wait a second. I'm not sure it was Hi at that time. It wasn't [Frederick R.] Kappel. It was Kappel, Romnes and then […] deButts. I think it was John [D.] deButts who was the [CEO] at that time, whom I did not know. I mean, I knew who he was, and I had shaken his hand, and what have you, but had no real time with him.

BROCK: Was it the president who made you that job offer? Or how did that arise?

TANENBAUM: Yes. It was the president. Oh, dear. A Texan, his name will come to me. Goodness gracious. Bill…[William L. Lindholm].

BROCK: It'll probably occur to you in a few minutes.

TANENBAUM: Yes.

BROCK: And what did you make of the offer? It seems like a very pretty wide mandate of things to be responsible for.

TANENBAUM: It was. It was very exciting.

BROCK: And back to…

TANENBAUM: And getting back closer to the…

BROCK: …that building…yes.

TANENBAUM: Yes.

BROCK: Was this, in a way, one of the top technical posts in AT&T?

TANENBAUM: It was. It was *the* top…

BROCK: The top job. So what was the job?

TANENBAUM: Well, the job had oversight of the part of the Bell Labs that AT&T funded, which was the Systems Engineering and Research [Divisions]. It also had responsibility for all the engineering that the telephone companies did: […] the design of the network <**T: 05 min**>, all the installation and operation aspects of the network, and of the…essentially, of the equipment, everything except the equipment that was on the customers' premises—the PBXs and telephones. But it had responsibility for all of that. [We] developed all the so-called Bell system practices [for the network], which were essentially the operating guidelines for everything associated with that, not how to install a telephone, but how to install a switch, and how to maintain switches and all the…everything that had to do with the network.

BROCK: And who…what sort of group of people did you have as directly working for you to do the job?

TANENBAUM: They were, I'm going to say, between 90 and a 100 percent technical and closer to 100 probably than 90. A lot of them from Bell Labs, because that was the path for Bell Labs, [for] people out of Bell Labs. Not Western Electric, […] unfortunately. But those groups were staffed primarily by Bell Lab…people who had started in Bell Labs. There was a steady flow of people from the operating companies in and out of that group. In fact, there was a steady flow of operating company people, for the most part, made up most of the staff. AT&T did not do a heck of a lot of outside hiring for telecommunications management. You get over into finance and public relations, what have you: a lot of outside hiring personnel…a lot of outside hiring. But on the technical side, they were mostly staffed by people who came either from Bell Labs or from the operating telephone companies.

BROCK: Were you and your group also responsible for forward looking—planning—for the future of the network?

TANENBAUM: Sure, yes. Yes. A lot of the technical work there was done in the Long Lines Department, which was AT&T's only operating…AT&T [Long Lines] operated all of the long distance, all the interstate. Everything intrastate was run by the telephone companies. The American Telephone and Telegraph Company: Long Lines Division was the operating function there. They had their own Engineering Department. But again, I had the dotted-line responsibility to that Engineering Department, as well as the Engineering Departments of all the operating telephone companies. We were the final group that standardized and said, "This is the equipment that's going to be used by the telephone companies." We were the group that had to approve Western Electric's, essentially, agreeing…decision to go ahead with the new products, because [if] we didn't approve it, they couldn't sell it.

BROCK: Did you like this new role?

TANENBAUM: Oh, it was fun. Well, it was my first real contact with the operating part of the business, with the end-user.

BROCK: Who were your major interactions with? I mean certainly your group of staff that you worked with, but beyond them…?

TANENBAUM: Well, kind of similar to the Western Electric job, principal interactions were with the chief engineers of the telephone companies—twenty-three of them, I guess. And with the…for the person who I was, when I was in Bell Labs, namely the person responsible for [all product and systems development]. So it was Bell Labs on one hand and the operating telephone companies on the other hand.

BROCK: Right.

TANENBAUM: And Western Electric, of course, it was…my interactions there would be with the head of…the Vice President of Switching, Vice President of Transmissions, and [Vice President of Customer Products]. <**T: 10 min**> Those would be the contacts.

BROCK: This period of '76 to '78, was the big…was digital switching…that continued to be the big technological feature on the landscape?

TANENBAUM: Well, that was the…we didn't have a local switch at the time. It was a few years later before the local switch became available, but certainly the transition to digital, also the transition to photonics, to optical transmission, those were the big technical drivers, supported, of course, by lasers and semiconductors.

BROCK: Yes. I thought we might break the chronology just a little bit here in the transition to becoming, in 1978, when you become president of one of the operating companies, New Jersey Bell Telephone, to go over…it seemed to me—1978—that the semiconductor area, the silicon semiconductor area, had reached a certain maturity by that point. You know, we have microprocessors around at that point.

TANENBAUM: But they had made very little penetration.

BROCK: Very…yes. But just in terms of the technology, it had…

TANENBAUM: Oh, sure…

BROCK: …it was there…

TANENBAUM: Yes.

BROCK: …and there…

TANENBAUM: Absolutely, yes.

BROCK: ...to stay. So I had just some general, I guess, reflections that I'd like to ask you about on the semiconductor story. Then, maybe after that we could return to the chronological sequence. Just to think about the roles of individuals who had a training in chemistry to the development of the semiconductor story, at these different places that you had worked by '78. So Bell, at Western Electric, and AT&T, so just…I guess, what I'm trying to explore here is the extent of the chemists' contributions to the development of this area. So if you had any thoughts about that.

TANENBAUM: I was pretty far away from chemistry at that stage of the game. I was still a member of the American Chemical Society, read *C&E News* [*Chemical and Engineering News*]. But…and I would…my spare time reading, I kind of tried to keep generally in touch with what was going on in the chemical areas. I served on the boards of a few chemical materials companies: Kennecot [Corporation], Cabot Corporation, American Cyanamid. But in terms of the evolution and growth of the semiconductor industry, you'll find many better sources than mine.

It was interesting to me how chemists…well, Bill Baker, I think, was probably the first chemist who was President of Bell Laboratories. But really, I was pretty far away from the people in chemistry, and even from the…you know, knowing the people who were doing the heavy work in semiconductors.

BROCK: Well, if we just return to the period of time when you were directly engaged, you know, we talked about the middle, late '50s. I was just really struck by, at Bell Labs you have the development of all these fundamental ingredients, let's say, of this silicon technology from epitaxy to photolithography <**T: 15 min**> to, you know, it…and I was just struck by the preponderance of chemists who were involved in those developments.

TANENBAUM: Oh, sure. Well, look…Once the physical principle was discovered, once the transistor effect was discovered, most of the rest of it is chemistry. And that's true of the laser, too. Once the laser principle is discovered, then the reduction of that to something useful is chemistry. It's materials science, if you want to call it, which is chemistry, basically.

BROCK: Right. Yes. I hadn't thought about the laser in that way, but it is very much an analogy. Well, then to kind of continue that thread, if you see this area…well, there's this area where, basically, Bell Labs developed the technology, the silicon technology, and really serves as a laboratory through the…for the world, as far as I can tell, because of the consent decree and licensing of it, and disclosing the technology to the licensees. But after that period of time, it continues to be very active in semiconductor work—Bell Labs—but doesn't seem to maintain the same sort of absolute forefront dominance that it had in the period when you were actively engaged. I was wondering, one, if you thought that was an accurate description? And two, why do you think may have happened?

TANENBAUM: Well, I think the principal reason that it happened was that we were constrained in terms of what we could do with the technology. We continued to supply much of our own technological semiconductor needs. We always required the highest level of reliability, but we couldn't go into consumer electronics; we could put the electronics in telephone sets, which we did. But the big, wide market…we couldn't go into computers, except for computers we were going to use internally. So the market for what we could do was tremendously limited.

BROCK: So in a way, to try putting that slightly differently, Bell Labs continued to, quite well, meet the demands of its sort of proscribed market.

TANENBAUM: That's right.

BROCK: You know, but that…

TANENBAUM: I think Bell Labs led the technology. The only place I can think of where we were somewhat behind was in the local digital switch. That was not a technical…not held up by a technical obstacle; it was held up by an administrative obstacle.

BROCK: Or by the integrated circuit, is that what you mean, or the digital switching?

TANENBAUM: The digital switch, yes. I mean Bell Labs didn't invent the…

BROCK: Right.

TANENBAUM: And I think that was a failure, because we put together most of the…developed, were the first developers of most of the technology of the integrated circuitry, but…and we should have. The integrated circuits should have been invented here. […] that was a failure.

BROCK: Well, that is an interesting way to look at, sort of, the course of semiconductor development at Bell Labs through the '60s and '70s, as meeting its very proscribed legal…regulatorily-proscribed market.

TANENBAUM: I would tend to put it in an even more somewhat negative way of saying: it was prohibited from taking advantage of most of the opportunities from telecommunications.

BROCK: Right. But it…but for positive viewpoint, in terms of the work that the people here did, it made no sense…

TANENBAUM: They…

BROCK: …for them to work <**T: 20 min**> in other efforts beyond meeting that need.

TANENBAUM: Yes, sure. And the real problem there was that to put in the kind of effort you need, for example, to be really competitive in random access memory…

BROCK: Yes.

TANENBAUM: …you need volume. And the volume here was always constrained by whatever the telephone companies did. Now at one time, you go back into the '30s and what have you, the telephone industry was the principal consumer of electronics. But you come into the '60s and '70s, we were not a large part of it, by any means. If you don't have those marketing opportunities you cannot put the resources…you shouldn't put the resources into the development.

The microprocessor, for example: our market was limited, just a tiny part of the total market there. We could never have sustained the development. And that was a real argument here at one time, as to whether we should go make a real run for the microprocessor. But the limiting factor there was we just did not have the market to support both the development and then the maintenance of generation after generation of that.

BROCK: Fascinating. Well, perhaps we could switch back, then, into the chronology to 1978, and leaving the top technical post at AT&T to running one of the operating companies. What was that telephone work…call like, or conversation like?

TANENBAUM: Well, it was just a call from John deButts saying that's what they wanted me to do. I said…my statement was, "Well, let's give it…I'm happy…if you think I'm the person for it, I'm happy to give it a try." Because it really…that's where the real telephone business was at that stage of the game. It also had the very attractive aspect that I didn't have to move.

BROCK: Can you tell me a little bit about New Jersey Bell as a company at that time? I really don't know.

TANENBAUM: It was what you'd call one of the middle-sized companies. Its business depends upon the population of the state, so it's one of the middle-size states. Its close proximity to Bell Labs did make it one of the places where there was some of the important new products got…the electronic switching, first system was installed in Succasunna, New Jersey, and, of course, the Bell Labs people lived here for the most part. It had its ups and downs. It was…the relation to one of my first jobs was to try to improve the relationship with the Public Utilities Commission here. The company had gotten at odds with the Commission. Being the head of a telephone company, I learned was a people job, not a technical job. The technology was for the most part determined by AT&T. You were expected to and did have input into what the needs were, and they depended on the telephone companies to tell them where the problems were, and where the opportunities were. You had your own engineering department to run.

But for the most part, both…there was an operating vice president who had the responsibility…he was probably the Number Two officer, really, and [had] all the operating responsibilities. There was a Commercial Department, then called the Marketing Department, who did what marketing we did, which when you introduced new products, telephones in particular, was a marketing job to do. One of the big jobs that we <**T: 25 min**> had when I came there is the whole casino business of Atlantic City was just really starting to develop. I think the first…there were no casinos in Atlantic City, but the first licenses had just been issued. So there was a really big market [potential] there. I had a marketing vice president who spent a lot of his life in Atlantic City.

Then, much of the rest of it was community relations. One of the things that really struck me, when I first became president…in fact, there was an article in the local paper, that New Jersey Bell had a new president. I was out in my shorts and a t-shirt at [a] gardener's market on Route 10. The guy comes up to me and says, "Say, are you the new president of New Jersey Bell?" I suddenly realize…I looked around the market: everybody in that market was my customer, right down the street. And [I realized] everybody there, at that time, everybody there was your customer.

So that's what a lot of the life was. You were out two or three nights every week going to a dinner here and a dinner there, and what have you, getting to know the governor [Thomas Kean]. […] I guess we were the second largest employer; the governor was the largest employer. So we were the largest non-governmental employer. The Bell system always had been very, very active in community [relations]. We encouraged our people to engage in local politics if they wanted to, and join community service organizations, and what have you, and getting to know the other industrial leaders in the state, who were our biggest customers.

BROCK: Right. Did you have to do a lot with labor unions?

TANENBAUM: Yes. Although, the bargaining…we had two unions, the CWA [Communications Workers of America] and IBEW [International Brotherhood of Electrical Workers]. New Jersey Bell was one of the organizations […] where the IBEW…one of the few companies where the IBEW was a larger matter than the CWA. But the bargaining was always done centrally, and really led by AT&T. The principal Bell System guy at the table was an AT&T vice president. So you had to deal…you know, you dealt with local matters. I don't think there was a contract…well, there had to be. I guess I was there long enough for one contract to expire. But I don't recall a problem with that. When I was at Western, we had a problem. We had a strike, and what have you. So that wasn't all together new to me. But labor, per se, was not a major issue.

One of the big issues that did come up was our Kearny plant, which was the number two plant in Western Electric, and probably the largest single site employer in the state. It was phasing down. So I had to help the governor understand what was going on there. I told him, I said, "Manufacturing is not the real future for New Jersey. It's not really the future for the northeast part of the country. You ought to aim toward making this the headquarters state, because we're right adjacent to New York City, which is *the* headquarters city. And also, the pharmaceutical business, which is not really manufacturing, it's R&D and business…

BROCK: Right.

TANENBAUM: You had Bell Labs here. So it's those kinds of services that you want to focus on." He was still worried about the people that were being laid off at Kearny <**T: 30 min**>. But it was community…strongly community and political job. Again, it was very comforting to have some understanding of the technology […].

BROCK: Sure. Were any of your…do you know if any of your peers in…the other presidents of the operating telephone companies, did they come from a similar technical background?

TANENBAUM: They may have had a technical background, but it would have been through the operating company…engineering: being an engineer, being hired as an engineer.

[END OF AUDIO, FILE 2.5]

BROCK: Did you enjoy that, community relations, political…?

TANENBAUM: I enjoyed some aspects of it. And other aspects of it: not what I would have chosen. I must say it did get kind of tiresome at times.

BROCK: Yes. Just…?

TANENBAUM: Just all the exposure, all the exposure to the public and media, and….

BROCK: So then in 1980, when you became Executive Vice President at AT&T, was that…did you find any shelter from that storm of public exposure, or did it get intensified?

TANENBAUM: Oh, no, not at all. The political exposure was increased, because I had responsibility for all of the administrative activities at AT&T, which included personnel, public relations, government relations, all of those kinds of undertakings.

BROCK: Was this another phone call from the President of AT&T…

TANENBAUM: Yes.

BROCK: …saying, "This is what I'd like you to do next?"

TANENBAUM: "It's time to come back."

BROCK: How did you…did you look with excitement at that new role?

TANENBAUM: Well, it was an exciting time. [There is a] Chinese curse that [says], "You should live in interesting times." This was an interesting time because the antitrust [suit]was very active.

BROCK: And this was a suit that was originally brought decades earlier or something like that?

TANENBAUM: Oh, I don't remember the exact time, but it was at least ten years earlier.[16] But it was going into trial then. I was a witness. So that was…I had written testimony. This was my third testimony, I think. I had first written testimony when I was in Western Electric about R&D. This was certainly at least the second time. I think there were two times of testimony that was written, and then just never used because it was out of date by the time the trial actually got underway. So I was involved with that.

There was also an effort to try to get legislation to head that off in Congress, the whole antitrust suit […]. I was responsible for trying to get that passed. We got it into the Senate, but then it just sort of stalled. So I had a lot of different experiences of the kind I never had before.

BROCK: What was really the argument of the antitrust suit? I mean weren't…wasn't the whole system a regulated monopoly more or less…

TANENBAUM: Well, it was…

BROCK: …at the time?

TANENBAUM: …it was regulated, that's right. But it was not a legal monopoly; it was a *de facto* monopoly. The FCC had decided to experiment with some competition in the parts they were responsible for, which essentially was all the interstate business. Each state has its own regulatory body for the intrastate. So they let this company called Microwave Communications Incorporated or some…it became MCI, into the private line business. Private line is when a company wants to hook up to its location or whatever, and the dedicated line…. It was, of course, an AT&T development that made that possible, and they made microwave communications, microwave radio, because they could never have gotten, I don't think, the licenses to build land lines. But with microwave, you [only need a small plot of land every] twenty miles or something. And so <**T: 05 min**> the FCC had let that start and then that company was clever enough to figure out how to use those [private] lines for just ordinary long

[16] United States v. American Telephone & Telegraph Co, Civil Action No. 74-1698 (D.D.C. 1974).

distance calls, by essentially establishing telephone numbers at each end […], and having people call in to that telephone number, and then, dial the number that you wanted to reach.

So that went to the FCC, and the FCC said, "No. They were not authorized to do that," and that's all they said. So AT&T decided to disconnect them because they were…disconnect them from our service because they were doing some things they shouldn't do. That caused a hell of a big stink, because it put them out of business temporarily. I think it was that incident in particular that really stimulated the Department of Justice to do that. I've forgotten the name of the guy who was the assistant attorney general for antitrust. I remember so vividly an interview he had on television, when he was asked, "Well, now, Mr. Attorney General, what benefit will this have to the public?" He said, "I don't know." He said, "That's not my business. And they're a monopoly. And I'm enforcing antitrust laws." That's not [an exact] quote, but essentially, that's what he said.

BROCK: So all of that government relations was with you at that time…

TANENBAUM: Yes.

BROCK: …in this period from '80 to '84.

TANENBAUM: Yes. I was very much involved in the final consent decree.[17] I was pretty much the technical consultant with regard to that aspect—what aspects would work, and what wouldn't, what could we live with, what would we have to do.

BROCK: This was in the 1982 timeframe…

TANENBAUM: That's right.

BROCK: …the consent decree?

TANENBAUM: That's right.

BROCK: That called…that AT&T would divest from the operating companies, is that right? Or sell its stake?

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[17] United States v. AT&T, 1982-2 Trade Cas**. (**CCH) P 64900 **(**D.C. Cir. 1982).

TANENBAUM: Yes. The original suit was to split off Western Electric. But around 1980, I guess, sometime in that timeframe…probably was 1980, because it was a change of administration, and a new assistant attorney general came in. Our general counsel, in talking to him, the new assistant attorney general [William Baxter]—as I've been told the story, you know I wasn't personally there—said, "This is the wrong suit." He said, "The real monopoly is between the telephone…local service and the long distance service." He said, "As far as equipment manufacture is concerned, there'll be competition. There's nothing to keep the telephone companies from buying other people. So this is the wrong suit." From that came the…in the meantime, the trial was not going well.

BROCK: For the government?

TANENBAUM: For AT&T.

BROCK: Oh.

TANENBAUM: The real concern was that if we ended up with a judgment, and the judge would become the person to restructure the Bell system, that we didn't know what we would end up with. I think Charlie [Charles L.] Brown, who was the CEO at that time, decided to…"Well, let's see if we can't find a settlement, which we think we can live with," and so the consent decree.[18]

The basis of that settlement was that the local telephone companies would be restricted to local service [and] would continue to be regulated; AT&T would not be in the local service business. It would be in long distance business, where the systems [can compete]. So that's where the competition is a natural monopoly: in the local service. Nobody else is going to rebuild all those wires. But in the long distance service, where it's microwave, optical, and what have you, so that's where the competition is. So that's the deal [that] was finally struck. Of course, that deal did not hold up. Local companies are in the long distance business…

BROCK: Today. Yes <**T: 10 min**>. So did that constitute…after that consent decree in '82, or the agreement in '82, was working that reorganization the number one thing on everybody's plate. That must have dominated things…?

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[18] Ibid.

TANENBAUM: It did. It did. I think the estimate was, at the peak, we had something like twelve thousand full-time people figuring out how to take the network apart without disturbing service. Taking buildings full of equipment, drawing yellow…painting yellow lines, essentially, in buildings saying this part is long distance, this part is local, which you could never really do, but it just said who is going to be responsible for this and what have you. But, that was the principal job, and I was given the job of figuring out how to run the long distance after the separation.

BROCK: Which was going to become the real locus of competition…

TANENBAUM: That's right.

BROCK: …going forward.

TANENBAUM: And the principal source of revenue for AT&T.

BROCK: Right. So how did you…just interested to know, you know, with a problem like that that falls in your lap, how do you do that? How do you…what do you do?

TANENBAUM: Do it the best you can. You get the best people you can find, and here the…fortunately, we had that in the Long Lines, what used to be called the Long Lines Department of AT&T, because they designed and operated the long distance [interstate] network. But there were a lot of decisions that had to be made in terms of where does local stop and long distance start. This resulted in…this was embodied in the consent decree dividing a country up into LATA—Local Access and Transport Areas, something like that. The country was divided up into a hundred or so of those. Both sides, local companies and the long distance [side], had to be involved in where are the natural places to draw those lines. So the judge had to approve those. Then you had to decide how to break up the network [but still keep it operating]. Described as…to someone as trying to turn a 737…dismantle or rebuild an airplane while in flight, essentially.

BROCK: Yes.

TANENBAUM: A 747, yes, because a 747 into two 737s in flight. You know that was the job. We had a lot of people [working] in an attempt to get it done, a lot of disagreements and discussions and what have you. Local telephone companies were, for the most part, not very happy about the deal that was finally signed. They turned out to be the ones who prospered by it. It was just…did the job day-by-day.

BROCK: That must have been the major technical problem…

TANENBAUM: True, and…

BROCK: …facing the company?

TANENBAUM: …an operational problem. There was no technical innovation that I'm aware of.

BROCK: Also, so what was the disposition of Western Electric?

TANENBAUM: It stayed with AT&T.

BROCK: Okay.

TANENBAUM: It stayed with AT&T. Its problem then became one…the operating telephone companies, I think, uniformly really didn't like the idea of being cut out of the long distance business. Because the long distance business really was the moneymaker. Local service has always been, and still is, subsidized from the long distance revenues. That's come down a great deal since the business has gotten less profitable. So they were concerned about how they were going to make a living.

I never really fully understood that, because the local…they were going to have to…everyone's going to have to file a case <**T: 15 min**> with the local PUC and go through and get the local rates increased. But it seemed to me that was not fun, but doable. But they wanted to get back…from the go-down, they wanted to get back in the long distance business. Now, of course, they've succeeded in that. It took fifteen years, but…

BROCK: Was it a political effort […]?

TANENBAUM: Oh, sure. Oh, absolutely. They had most of the political contacts.

BROCK: For the very reasons we were mentioning earlier…

TANENBAUM: That's right. That's right. We also had the job of…everybody knows what AT&T is now. But we did some polls, some market research, fairly broadly across the country. Guess what the percentage of people were who said they knew what AT&T was, and then were able to tell you [what AT&T did]?

BROCK: I don't know. Half.

TANENBAUM: Four percent.

BROCK: Four.

TANENBAUM: Yes.

BROCK: Because it seems just ubiquitous now.

TANENBAUM: Yes. So we had to put on what was probably one of the largest marketing campaigns in the time remaining to get the AT&T…. They knew what New Jersey Bell was. But AT&T, "I never heard that. I'm not sure." The stockholders probably knew what it was, but probably not all of them. So that was a big part of the job.

One of the fun parts of that was that the Olympics were being held in Los Angeles. We got the opportunity to [sponsor and operate] the first [nationwide] torch carrying [role]. All the Olympics do that now, but we were number one, and AT&T Communications, my company, sponsored that. So that was a lot of fun.

BROCK: But, essentially, in this consent decree reorganization, Bell Labs would be unchanged, right, because the AT&T-Western Electric relationship was unchanged?

TANENBAUM: Well, it was changed to the degree that the telephone companies needed some of the technical talent of AT&T that Bell Labs had. They were going to be [independent] equipment purchasers now. No one was going to tell them what to buy, so they needed that [talent]. Then, for all the engineering in the local plant, designing the local networks and […] the equipment that's used there, they needed that [talent]. So about 20 to 25 percent of Bell

Labs was split off, and established in a new corporation called Bellcore [Bell Communications Research] that was jointly owned by the telephone companies, the Baby Bells, so-called.

BROCK: Was that something that was sort of planned as a solution one could live with in devising the consent decree…?

TANENBAUM: Sure.

BROCK: Because people…okay.

TANENBAUM: Oh, sure.

BROCK: All right.

TANENBAUM: That necessity was foreseen.

BROCK: Yes.

TANENBAUM: And the local companies no longer own Bellcore. It was located right here in New Jersey, because that's where most of the people live.

BROCK: Right.

TANENBAUM: But it was bought. The local companies decided they wanted to do their own engineering, each of them, so they built their own engineering groups. So now you have four—[there were] six or seven—It's now four Baby Bells. Some babies. Bellcore was sold to SAIC **[**Science Applications International Corporation], a large consulting company, government consulting company. Now it's called Telcordia [Technologies], I think.

BROCK: So when the reorganization became official, you became the first chairman and CEO of AT&T Communications, which was that long distance…

TANENBAUM: That's right.

BROCK: …element…

TANENBAUM: That's right.

BROCK: …that you had been strategizing for. So a brand new job in essence. What was it like running the long distance company in this timeframe? I mean it must have…it's a brand new environment, brand new…

TANENBAUM: Yes.

BROCK: I guess many of the same people, but a new structure…

TANENBAUM: Yes. The biggest challenge was working out new arm's-length relationships with the telephone companies <**T: 20 min**>, because the telephone companies had to deal with us the same way they dealt with MCI and everybody else. That was new. We got into a real problem on Long Lines, actually, in the first year…private lines. Because private…hooking up of a private line means dedicating a circuit from one customer location, or one customer, to another customer location. The two ends depend upon the local telephone companies.

That was always done…you're always looking for [an available local] circuit. You know, you got a new customer, you're looking for a circuit [that] Long Lines can connect […] to the local telephone company's office; and that's done mostly electronically through trunks—[we were] sharing equipment. So you can usually squeeze that in, but then they've got to find a dedicated line at the two ends. [And now it's not] one Bell system; [there]'s one telephone company over…Baby Bell here, and another telephone company, Baby Bell over here, and they all do things differently. They used to communicate and work together with Long Lines people and they'd do it. Suddenly, there's a barrier. They could no longer treat them the same way, because if they do, they have to treat them the same way they do when MCI wants to set up a customer, or Sprint, or any of the other several hundred telephone companies…new telephone companies.

It turned out that the record-keeping was not everything it should have been. So we had a lot of customer problems for six months, I guess. So one of the first things we had to do was get that straightened out. That network, in general, worked just to switch networks, worked just…worked just fine. But we had a lot of regulatory issues because the question was: how much do you pay these local companies for the last mile of what they put in? That was always a great subsidy to the local companies, so they created…the regulators created access charges and a long distance customer would pay *x*-teen cents per minute, which they used to…the local companies used to get, essentially, as part of one big company. So each of those had to be argued out state-by-state, essentially, and with the FCC. So that was what…you know, once I…up until the actual cut-off date on 1-1-84, it was all the planning and everything we were going to do. From then until the…—and I was on the job for about a year and a half after that—it was a question of making it work.

BROCK: Was there a big blossoming of competition in long distance at that time, were, essentially, competitors in the '82 to '84 timeframe getting ready for this change?

TANENBAUM: I think it had been a continuous growth process. But it took…the slope certainly changed discontinuously, essentially. Just when that happened, I'm…I don't specifically recall. A lot of the growth, and there were hundreds of new companies—most of them were so-called resellers. They did not build it onto the facilities; they bought wholesale [usage] from the existing carriers. There were really only three…at the beginning, three carriers with facilities. There was AT&T, MCI, and Sprint. Then the other people got in, some of the pipeline companies, Williams [Telecommunications Corporation] in particular. They had a lot of right-of-way.

BROCK: Oh, yes.

TANENBAUM: Optical fiber was now readily available. So they built their own networks. They've since gone out of business, but they did okay for a while. But most of the new companies were resellers. They would buy circuit minutes, essentially, and resell them.

BROCK: Did you find that year and a half…was that a stressful time for you?

TANENBAUM: Very hectic. Very hectic, yes. It was…you know it was fun, but it was hectic. Nothing boring.

BROCK: I can't even imagine. Then, I mean, what was the size of AT&T Communications?

TANENBAUM: About a hundred <**T: 25 min**> and fifteen thousand [employees]. We got all the [telephone] operators, and that was about half of that, about sixty thousand operators.

BROCK: Right. Yes. Do you think that, again, this was a case in which your technical background gave you an extra comfort level with what was happening, especially with the network?

TANENBAUM: Yes, especially with the division of the network, although I didn't find myself involved in many technical discussions. The engineering groups, Long Lines in particular, handled most of that. Then, as I said, it was mostly working out management details: who is going to be responsible for operating and maintenance for common facilities that we share, who is going to…how are we going to do that? How are we going to pay each other for it?

BROCK: Very complicated. So after a year and a half, you become Executive Vice President of AT&T again, is that correct? Or…?

TANENBAUM: No. My next job was as Vice Chairman of…Vice Chairman of Finance.

BROCK: Okay.

TANENBAUM: Yes.

BROCK: Then later, the CFO [Chief Financial Officer title] is added, is that right?

TANENBAUM: Yes. What happened was the CFO reported to me. He was moved to another job, and I didn't think we needed another CFO, so I just took on that responsibility as well.

BROCK: Now at this time, AT&T Communications was the long distance part of the company…or it was its own separate company?

TANENBAUM: It was a separate corporation, [but fully owned by AT&T].

BROCK: It was a separate corporation. There was Western…well, so what were the…when you became Vice Chairman of the Board of AT&T, what was AT&T, then, at that time?

TANENBAUM: Well, it was a lot of things. But the major components were…and AT&T Communications was the largest component. Network Systems was the second largest component; that’s what was Western Electric. Bell Labs still existed as Bell Labs and reported to the CEO of AT&T. There was a Customer Equipment Division, which was essentially the marketing arm for that part of…what was that part of? Probably part of Western Electric…Network…. Because we…yes, Western Electric had to market, now, its telephones, not just to the operating companies, but primarily to the public and sell PBXs directly to the public. Those were both very competitive businesses by that stage of the game. So the two [biggest] parts were Communications and Network Systems.

BROCK: Right. Were those wholly-owned then by AT&T…?

TANENBAUM: By us. Yes.

BROCK: Okay.

TANENBAUM: I had a Board of Directors who were all inside.

BROCK: Okay, right. So when you…well, can you tell me the story of, then, becoming Vice Chairman of AT&T in this timeframe, ‘86, I guess then? Or ’85?

TANENBAUM: That’s about right. Maybe it was the middle of 1985. It was not my favorite job. I would have much preferred—it was in a sense a kind of promotion—but I would’ve much preferred to stay with AT&T Communications, because that was much more of a hands-on job […].

BROCK: Yes. What was this vice chairmanship like?

TANENBAUM: Well, I was principally the chief financial officer and given the title of vice chairman. There were three of us, I guess, three vice chairmen—[the two others were Charles Marshall and Randal Tobias]. One had responsibility for all the administrative functions of AT&T. Let’s see <**T: 30 min**>. I think there were three of us. That was thirteen years ago. I’m trying to think of who they were. One was that…one had responsibility for Network Systems (AT&T Technologies) it was called […. The third one was Communications—the job I had had]. There were three of us. Yes, that’s right.

BROCK: This was less hands-on than the one before.

TANENBAUM: Oh, yes. Well, this…

[END OF AUDIO, FILE 2.6]

BROCK: Great.

TANENBAUM: And [it] was the [CFO]'s responsibility for all the [financial functions of] the company—managing the pension plan, and the AT&T savings plans, which totaled over…I forgot the numbers, but several tens of billions of dollars, [and for the budget activities of the operating segments]. It was interesting in a sense, but it was removed from the active part of the business.

BROCK: Did they…was the feeling that they wanted you to…or your colleagues wanted you to do this, because it was going to be such a new financial environment for the organization?

TANENBAUM: Well…but all the jobs were new.

BROCK: So you did that. Then the CFO designation, I guess, gets added to it in '88, and you carry on doing that through '91.

TANENBAUM: Yes.

BROCK: So it's that same sort of responsibility through there…

TANENBAUM: Right.

BROCK: What were the big changes? I mean, certainly '84 was a huge change. What were the changes for AT&T that you were particularly interested in over this through '85 to '91 period?

TANENBAUM: Well, we had a number of problems. One was, essentially, a regulatory problem, this whole question of access charges that we had to pay. At first our competitors didn't. Later on, they had…they now pay these same access charges. Major flow of funds between AT&T and the telephone companies through the access charges, billions of dollars annually, and the management of that. We had a lot of stockholders, of course; that was part of the finance responsibilities to the stockholder relationship. And then just the businesses themselves, the financial aspects of the various businesses themselves as to how AT&T Technologies was going to make out with an altogether different relationship with its customers. And really the need to try to build new customers. And the principal markets for new customers were foreign markets. Those were expensive to get into, so a lot of financial considerations that you have. Then a desire within AT&T to diversify; we no longer were required to stay in telecommunications…

BROCK: Right.

TANENBAUM: …and what businesses to go into, and how to pay for them, and how to try to judge as best you can what different financial opportunities are.

BROCK: Was the wireless communications part of the landscape then in '91…?

TANENBAUM: Well, we had…the local telephone companies kept up all of the wireless business. AT&T, after I retired, […] did buy a wireless company, became AT&T Wireless.

BROCK: Okay.

TANENBAUM: And a cable company [at a very high price], which was a big mistake. Really, it was a terrible mistake. It's a source, I think, of many of AT&T's financial problems.

BROCK: Because it was such a…that was…now that happened after you were…

TANENBAUM: Oh, yes. Yes.

BROCK: …gone.

TANENBAUM: So did the AT&T Wireless business, the acquisition. We went into the credit card business when I was there, which worked out okay, but was sold, I guess, after I retired, and I didn't have…it was probably the right thing to do. It just didn't have nearly the synergism that people thought it would have with the rest of our business.

BROCK: Well, to <**T: 05 min**> return to the cable deal, which, then, I guess, turned into the broadband, AT&T Broadband. Why do you think that was…was it just the sheer expense…?

TANENBAUM: Yes. Yes, [AT&T] paid much too much for it, and put a tremendous debt load on AT&T—a hundred billion dollars is what that cost. When it was spun off, your assets were more in cable business that was spun off than what remained in AT&T?

BROCK: So, well, maybe you could tell me a little bit about your decision to retire in '91. Was that…I would imagine from listening to you that that was a difficult decision to make.

TANENBAUM: Well, it really wasn't too difficult, as a matter of fact.

BROCK: Really.

TANENBAUM: I just wasn't really having fun anymore. I felt like I was too far away from the heart of the business, and, certainly, way, way away, farther than I'd ever been from the technical aspects of the business. I retired two and a half years early. My wife played a role there; she could tell I wasn't having any fun. I mean it was interesting enough, but I just really wasn't…I decided it was time to start doing things that I really wanted to do again.

BROCK: Well, I do have some questions about some of your activities that I'm aware of in the 1990s, after you retired. But I wanted to also, maybe, just spend a few moments about your thoughts of the experience of AT&T and its further divisions, subsequent to your retirement. And what, if any, advice to people who are running the show during that period, and asked you what…did you have any sort of continuing role with the company?

TANENBAUM: No. That was not a Bell System tradition. In fact, there was an absolute understood regulation that you did not hire retired officers as consultants, for example. When people retired, they retired. That has its good points as well as its bad points. I mean you do lose…you just continuously lose a good deal of knowledge of the company. On the other hand, because AT&T for the most part had always hired internally in the old days, you always had someone with a lot of experience coming up. I'm not…I don't know whether that really lasted. I don't know whether AT&T started doing something different or not in that regard. But I had no involvement in it.

I had sort of made the decision to retire early, a couple of years before I [actually] retired. In fact, I told my boss—I gave him about a year's notice—about the fact that I was thinking very seriously of retiring. My wife could always tell. Charlotte could always tell, she tells me, when I was sort of started feeling like I had a job in hand. When I took a new job, I would pretty much stop doing everything I was doing outside, but then I'd start picking those things up again. I really started doing that. So that retirement, the principal discontinuity was that I had to fly commercial rather than AT&T planes. I did a lot more traveling the first…I would say for the six to eight years after I retired than I'd done in the previous several years.

BROCK: Well, I know that you were involved, it seemed to me, quite extensively with both the National Academy of Engineering and the National Research Council in the '90s. So I'd be interested to hear about your experiences with those groups, but also, you know, I'm intrigued <**T: 10 min**> by that statement you just made about how much you were traveling. Is this just traveling for pleasure?

TANENBAUM: Oh, no, no. Especially…I was on an airplane, on the average I figured it was something over three times a month…almost every week I was on an airplane. It was mostly going…it was going to board meetings. It was going to Johns Hopkins, because I'm a trustee or [to] MIT as a trustee. I was on the Executive Committee of MIT, which met once a month, so I saw a lot of Boston. Then the various boards that I was on and I became…I did become a consultant at General Motors [Company] and a member of their Science Advisory Committee. So I sort of planned for an early retirement and it worked.

BROCK: Well, I did see…I was interested, also, to ask you about […] your participation on corporate boards. The list of corporate boards on which you served really goes across a wide range of different sorts of corporations. This whole issue of corporate governance is so vague today, I'd be interested to hear about what your experiences were serving on corporate boards and then how that experience formed your opinion on this whole notion of corporate governance.

TANENBAUM: The boards I served on all had pretty strong technical bases. Now you might not think that of State Street [Corporation], which is a financial institution, but their principal business is information processing. They are the largest custodian of funds for mutual funds in the world. Their holdings are a few trillion dollars in terms of…and that's a big, big information processing corporation. They had very, very few branches, and so they are primarily an information processing company.

BROCK: Right.

TANENBAUM: All the other boards are very strongly technically oriented. I think that's what attracted me to them, and made it fairly easy on me to understand the companies and all of what was going on. I have to tell you that I guess, maybe, I was just lucky. But in all of those boards, I felt that the board of directors really knew what was going on in the company. Now, you couldn't tell whether there was someone down in the company doing something illegal or not, but [when it] happened, [you] learned about it immediately. That was just built into the board. Could we have been fooled if the top management had been lousy? I think it would have been difficult, just because the boards dug rather deeply in to the companies and what they were doing, and perhaps easier in a technical company than it is in others.

But I was on, I think, really only one board where we had a problem with the CEO, and had, essentially, to fire him. But we did. The problems were, in that case, not questions of fraud or what have you, just a very capable guy got interested in other things and just wasn't paying enough attention to the business itself. But for the most part, the companies were well managed.

I was involved in a number of searches for CEOs when <**T: 15 min**> these people retired, because I was on some of the boards a long time. But I cannot understand a situation like Enron [Corporation] where the CEO can take the position that, "I didn't know all of this was happening," when what was happening was so deep and large and fundamental to the business. So I find that difficult to understand. I think, generally, corporate structure and governance, I don't see anything systematically wrong with it. But you've got to have active people who feel their responsibilities and are willing to spend time to dig into them.

BROCK: Did you see your role on these boards of directors as being a knowledgeable reality check or tough questioner for the sort of senior executives? Or what did you see your…?

TANENBAUM: Well, I felt my role was, essentially, to represent the stockholders and understand for them, try to understand the business, try to understand where there were problems that were affecting their investment, and the reasonable expectations, and do something about them.

BROCK: Was that…do you know if that was a stance that your colleagues on these boards shared, seeing themselves as the shareholders' representative?

TANENBAUM: For the most part. I mean, I won't claim that everybody on every board I was on was always doing the job. They probably had a similar opinion about me. But for the most part, I think the boards I was on took that responsibility very deeply, and had no problem asking very tough questions of the CEO, wanting to know, [for example], why last year wasn't nearly what we expected.

BROCK: How does that differ from how you thought of your role as a trustee for Hopkins and MIT? I mean there you're certainly not representing stockholders. Are you representing some different stakeholders or…?

TANENBAUM: Well, you're…first of all, you're representing, I would say, the stakeholders. You don't have stockholders, but you do have a number of different kinds of stakeholders. You had the people who have made contributions to the institution and created an endowment for it, which ought to be both well managed as well as properly used. Certainly, the immediate big stakeholders are the student body. And then the faculty is the continuing group that's dedicated their careers to the institution. So you have…you represent all of those things, I think.

The institutions…I've been fortunate enough to serve on quite successful institutions who have outstanding reputations and your responsibility is to make sure that that is maintained and hopefully improved if you can. The jobs are not really all that different. Finances are an important part of those institutions. You've got to make sure that they're properly managed.

BROCK: Well, could we talk a little bit, then, about your…the Scientific Advisory Committee from General Motors, another very large industrial concern? You did that in the late 1980s, was that right, if my memory's correct?

TANENBAUM: No. It was after I retired.

BROCK: Oh, was it.

TANENBAUM: Yes.

BROCK: Okay. I'm not familiar with what this group does.

TANENBAUM: Well, it's a group of about a half dozen people. It's interesting in that it's a committee of the Board of Directors <**T: 20 min**>. Now, in actual fact, I guess, when I first went on, there was a board member…I'm going to have trouble remember the name [Thomas E. Everhart]. He was the president of Cal Tech [California Institute of Technology] at the time; [he] preceded David Baltimore, who is now the president. Who was…sort of oversaw the committee, but that changed very soon after I joined it, for reasons I don't really know. In fact, it is a management…it is in practice a management advisory board. But it makes its reports to the very top management, the CEO and his senior officers. Again, it was not so much a science advisory board [as it was a technology management advisory board].

I think the board—and it's been in existence for, I think thirty or more years—it was started back around the time when industry thought maybe it ought to be getting more involved, I think, in more basic research. I think it was, at its beginning, kind of an oversight committee to advise the R&D laboratory. When I came on, it was changing its complexion rather substantially. Its chairman, by way at the time, was a chemist, George [M.] Whitesides of Harvard.

BROCK: Oh, yes.

TANENBAUM: The mode that it was operating in then was that it picked a particular subject, technically oriented subject—not necessarily science oriented, a technically oriented subject—in conjunction with the agreement of the management and investigated that subject for its pertinence in its operation at General Motors. I was brought, on I'm pretty sure, because the subject they had picked that year was the relationship with EDS. Now, I don't know if you know anything about that or not. EDS, Electronic Data Systems, I guess, big computer company founded by [H.] Ross Perot, a name that you've probably heard.

BROCK: Sure.

TANENBAUM: And bought by General Motors back, I think, it had to be in the early '80s, to take over all their computer operations, operate them for them. [They] essentially brought in the company to outsource all of their information processing systems. Actually, at that time, we were in discussions, AT&T was in discussions with EDS about some kind of partnership or not. GM came in and did that, which really surprised us, I guess, because I know—I for one, and a number my colleagues—couldn't understand an organization turning over all of its processing responsibilities to another company, even if it owned them. And particularly, since the agreement that was struck was that they would continue all their other outside business, and they'd just take on GM as another client.

Well, that wasn't working very well. They asked this Science Advisory Committee to look at it and tell them what they thought about it? So we did a year's study. Shortly thereafter, GM decided to spin off EDS, because it really, really wasn't working [well for GM]. They were treating GM just as another…as a captive client, essentially. Each part of the company would have its own client in GM, and they would sell the services to that client. I know, I particularly…I went around and started asking [EDS people who serviced product development], "Well, how do you tie marketing into product development?" [You'd get the answer], "Well, that's somebody else. You'll have to ask them. They have the responsibility. We'll bring them in. You can talk to the people who have responsibility for marketing." There's just no communications whatsoever for a kind of technology that requires joint system development.

We did a number of similar things. One was on a technical education for the whole GM engineering staff. And <**T: 25 min**> I think the current [Science Advisory Committee] chairman is a fellow named Heilmeir, who was another chemist, who invented the LCD, liquid crystal display technology at RCA. He was also head of Bellcore. He was the second head of Bellcore.

BROCK: What is his name?

TANENBAUM: George [H.] Heilmeir, I think, very interesting guy.

BROCK: Sounds like it. I'm sorry, you were saying that the current topic […]…

TANENBAUM: He's the current chairman. I have no idea what their current topic is.

BROCK: I saw that you also, I think in this post-'91 period, were involved with a group concerned with getting better equity for minorities in engineering, in the sciences, is that correct?

TANENBAUM: Oh, yes. Yes.

BROCK: Was that a longstanding issue that you saw back to when you were recruiting for the facility in Hopewell?

TANENBAUM: Well, sure. That organization was started, I suspect, back in the '60s. It was started by the then current CEO of General Electric [Company]—whose name will come to me, if I think about it—who got together a half dozen of the major technically-oriented corporations, their CEOs, and said, "We need to do something about this." So they agreed to set up NACME, National Action Council for Minorities in Engineering, and to put their senior officers on the board. They [each] contributed something in the neighborhood of a couple hundred thousand dollars a year, primarily for scholarships for minorities—minorities being Afro-Americans, Hispanics, and Native Americans—in engineering.

I became the AT&T representative and we all kind of rotated the chairmanship. I was [chairman] for a term, with a group that, essentially, was trying to search how one can attract—and more than attract, maintain—minorities in engineering education and then on into jobs. The organization still exists, and I think it's pretty successful, if you simply look at the numbers that…you can look at the numbers of their baccalaureates, which has gone—still too low—but it's gone up significantly. As a significant fraction, not all of them by a long shot, but a significant fraction of them were NACME scholars.

BROCK: Right. I guess the second to last thing, we need to touch on in this period of '91 to the present, is your moment with the National Academy of Engineering [(NAE)] and the National Research Council, if you can describe that for me.

TANENBAUM: Sure. I don't know how much you know about those organizations…?

BROCK: Familiar with both, but not the deep inner-workings of both.

TANENBAUM: Well, I've been…I was a member of the NAE for a long time. I think I was elected in the early '70s. The academy was a good deal smaller then. I think it was founded in '64, something like that.

BROCK: I didn't realize…

TANENBAUM: Well, it was branched…it was formed from a branch of the National Academy of Sciences. They are sister organizations now. It was during my first few years [that] I was asked to become the NAE representative on the governing board of the National Research Council. National Research Council <**T: 30 min**> is really the arm that does all the work for the National Academies. So I served. That happened while I was up in Boston, because I commuted once a month to meetings of the governing council, which finally approved on all projects that they take on. Then, I became […] a counselor of the NAE, which is sort of the board of directors of the NAE, and served two terms. Then I became relatively inactive for a period of time. It was just about the time that I was starting…

[END OF AUDIO, FILE 2.7]

TANENBAUM: I was asked whether I'd be willing to run as vice…become the Academy's […] Vice President […]. That was one of the things that came up at the time [when I was seriously considering an early retirement from AT&T], I said, "Well, sure." I figured I'd have some spare time. Little did I know what would happen. And I went again on the governing board of the National Research Council. It was during […] my second term as vice [president]—you're permitted two terms, and you're out as an officer—that our president retired and we had an election for the president. We nominated our candidate and there was another candidate who ran as a write-in, not as a write-in, but by petition, and he won by just a few votes. It was a guy I had known for a long, long time, and I couldn't understand how they could elect him. He was a disaster, absolute disaster, terrible. Something had happened to the guy, I told my children.

But I won't go into the gory details, but the board finally had to impeach him, essentially. Except we discovered that we did not have in our charter, our by-laws, a clause that permitted impeachment. So we had to get a change in the by-laws and have everybody vote on them. Then we impeached him. But during that time, I effectively acted, long distance, as the president of the NAE. I was on the phone, I can't tell you how many times a week. Fortunately, I could do most of it by telephone. But now we've got a guy [William A. Wulf] who is now in his second term, doing a terrific job. So that [crisis took] a good little more of my time than I had expected. But overall the experience was one of the more interesting, because the work that the National Research Council does is taken very, very seriously by [U.S.] Congress and by the administration, and does, I think, have an important impact on the country.

BROCK: Have you…did you participate in any Research Council efforts personally to work on…

TANENBAUM: Oh, I was…

BROCK: …particular projects?

TANENBAUM: …I was on a number of study committees. I was a member of the Materials Research [Materials Advisory] Board (is that the name? Something similar to that) for many years, and led a study on innovation. "How do you move a new discovery into practice?," which was a published report, well out of date now. But what we did was get about a half dozen people who had been closely associated with major breakthroughs. [Each of us] had to write individual chapters. I did the chapter on superconducting magnets. We had someone from GE who did the chapter on ultra fine particle [high] coercive force magnets. Someone from Corning, [Incorporated], on Pyroceram. And there were about half dozen things.

One of our great findings was that there is no clear path to discovery [and] innovation. If you look at it all, backwards and forwards, it stops and starts, at least the things we looked at…they were unpredictable, but generally required some one individual who pulled them through, finally made them happen.

I was also, a member of a study that led to the…what do they call it, the establishment of Technology Assessment Board that was established for the Congress by the Congress. I guess [that] lasted up till [President Ronald W.] Reagan. I think Reagan finally discontinued it. But it fully supplemented the president’s advisor, scientific advisor. It was the Congress’s arm to look at proposed new legislation, and <**T: 05 min**> what its effect might be on technology.

BROCK: Well, the final thing I wanted to ask you about in this period of ‘91 to the present, were other important social or community activities or philanthropic activities that you’ve been involved with that you’d like to talk about.

TANENBAUM: Well, the first thing that comes to mind, when you say that is our Performing Arts Center in downtown Newark, [New Jersey]. I don’t know if you’ve seen that at all, but you probably haven’t.

BROCK: I haven’t.

TANENBAUM: But while I was [at] AT&T—in fact, again, it was just shortly before I retired—I got a call from the governor, Tom Kean, who is a famous name now, […who] was very interested in the performing arts himself, also interested in trying to get Newark back on its feet, and asked whether…. I’d been fairly active in—and my wife also—on matters of the performing arts—she [had been] on the board of the [School of the] Garden State Ballet, and I was on the board of the New York Philharmonic, and she was [then] on the board of the symphony, New Jersey Symphony [Orchestra]. [Kean] asked whether he thought it would be both useful and doable to establish a world class performing arts center in New Jersey. In fact, he asked me whether I would chair that, and I told him I thought [an arts center in Newark] was a good idea. I thought it really…he was hoping it could be something—an event to start to get Newark on an upward turning track. [But I also told him] that he really needed a private individual of large resources [as chairman] who could make a leadership gift to get it started, because private funding was going to be very important to being successful.

But I did become one of the founding directors and led the campaign to raise money from corporations in New Jersey. We opened it [New Jersey Performing Arts Center] six years ago, and had audience of five hundred thousand people in our first year. And the real estate market started picking up. People came back to [the Arts Center in] Newark who had not been in Newark in thirty years. And Newark is on a growth path now, I think. I think many people will agree that [the Performing Arts Center] was an important part of the trigger. So now we don’t have to go into New York to see first class entertainment, and [I] get some [personal] satisfaction from [that] as well.

BROCK: Great. Well that completes my list of chronological questions. I guess I would just ask you, if you think there’s something very important that I’ve…in your experience that I’ve overlooked that’s fallen through the sieve of my research, or if there’s any other comment that you wanted to make at this time?

TANENBAUM: I think you've been very thorough. You've given me the cues and the opportunities to talk about all the things that I can think of that I wanted to talk about.

BROCK: Okay. Well, I'll just…

TANENBAUM: And I feel a little talked out.

BROCK: I'll turn it off, then.

[END OF AUDIO, FILE 2.8]

[END OF INTERVIEW]

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